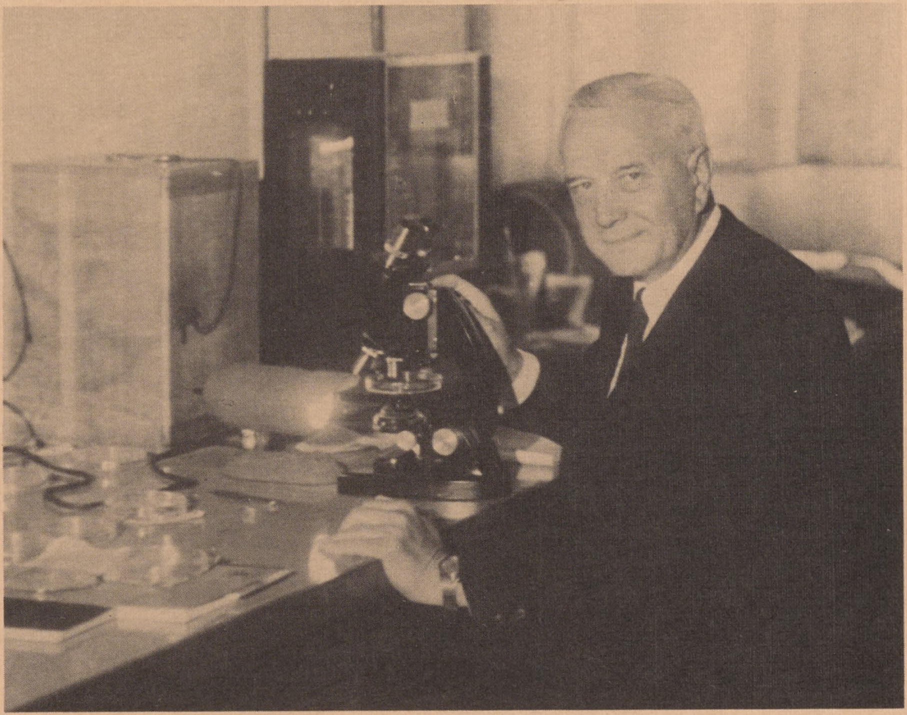
UNIVERSITY PLANT PATHOLOGIST at DAVIS
MEMOIRS of LYSLE D. LEACH



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Above: Portrait unveiled at the dedication ceremonies for Lysle Leach Hall, October 22, 1982.

Front cover: Lysle D. Leach conducting laboratory research at the University of Concepcion, Chillan, Chile, October 1962.

Back cover: An aerial view of the University Farm campus in 1928, including in the upper right hand portion of the photograph the two earliest buildings housing the Department of Plant Pathology.

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UNIVERSITY PLANT PATHOLOGIST AT DAVIS: MEMOIRS OF LYSLE D. LEACH

Preface by
James B. Kendrick, Jr.
Introduction by
Roy Bainer
Interviews by
A. I. Dickman and F. J. Hills

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STATEMENT OF EDITORIAL PURPOSE

The procedures of oral history projects include not only the tape recording of memoirs but also their transcription, editing, and eventual production in book form. What is presented to the reader is a version of the spoken word, and overt attempts to mask this fact rob the presentation of the intimacy, candor and spontaneity which give each memoir freshness and charm. However, standard and recognized editorial techniques are used to maintain a consistency of style throughout all oral history project publications. Since basically each title is for University archival deposit, such matters as dates, names, places and scientific terminology must be presented with the utmost precision. Editors will rely on *The Chicago Manual of Style* (1982) and the project's own style sheet to maintain an optimal printed version of the spoken word.

BIOGRAPHIES OF THE INTERVIEWERS

AVROM I. DICKMAN was born in Buffalo, New York in 1910. He received a B.A. degree from Pennsylvania State University in 1932. He was in sales management for Equitable Life Assurance Society from 1932 to 1970 when he retired. Shortly thereafter he moved to Davis and became a volunteer editor of the newly organized Oral History Office at the University Library, UC Davis. He served as head of the University's oral history program until his death on August 26, 1981. He also served as president of the Friends of the Library, University of California, San Diego, and of Library Associates, University of California, Davis.

FREDERICK JACKSON HILLS was born in Oakland, California in 1919. He received a B.A. degree from UC Berkeley in 1941. After working for Spreckels Sugar Company for a number of years, he enrolled in graduate school at UC Davis, earning a M.S. degree in 1951 and a Ph.D. degree in 1961. Since 1951 he has been associated with the UC Cooperative Extension Service, holding the title of Extension Agronomist in the Department of Agronomy at UC Davis.

CELEBRATING 75 YEARS OF TEACHING,
RESEARCH, AND PUBLIC SERVICE

During the 1983-1984 academic year, the Davis campus of the University of California is celebrating seventy-five years of service to California—service expressed in teaching, research, and public service.

Long renowned for its strengths in the agricultural and biological sciences, the campus is more and more recognized for the quality of teaching and research in the social sciences, arts and humanities, physical sciences, engineering, medicine, veterinary medicine, and law.

Reflecting on its achievements and mindful of its traditions, the campus continues its dedication to excellence in teaching, the pursuit of truth and knowledge through research, and the dissemination of information through public service activities.

The Davis campus grew out of legislation passed in 1905, establishing a University Farm where young men and women could combine scientific "whys" with technical "hows" in agriculture. The land for the campus was purchased in 1906, and the first students came to Davis in 1908, some for limited course work and some from UC Berkeley for practical training in agriculture. From these rather limited beginnings, the campus has grown to its present size (3,550 acres) and enrollment (19,000 students), offering a full array of undergraduate and graduate work, research in every academic discipline, and public service programs reaching out to the general public throughout the state, the nation, and to many other countries.

The Oral History Office, UCD Library, is proud to offer this volume, which sheds light on much of the early history of the Davis campus, in commemoration of the seventy-fifth anniversary of the Davis campus of the University of California.

DEDICATION

Jane and I dedicate this volume to Avrom I. Dickman and to Joann Leach Larkey, who were principally responsible for initiating and completing the project.

Dick Dickman, after a successful career in the insurance industry, retired to Davis, California in 1970 and volunteered to organize and administer the Oral History Office in the University Library on the campus. Under his leadership, some thirty oral histories were published between 1970 and 1982. Dick was also a close friend and my congenial partner at golf and bridge.

On my eightieth birthday Dick and Joann announced that I would be participating in an oral history. When Dick was seriously ill with cancer, Joann and I promised that we would complete my oral history. Therefore, this work is one of the last completed under Dick Dickman's guidance.

Joann Larkey completed the remaining interviews after Dick's passing and did all of the editing leading to publication.

LYSLE D. LEACH
July 10, 1983

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PREFACE

MY RECOLLECTIONS of Lysle D. Leach span more than half a century, dating from the time when he and his wife Jane came to Davis in 1929 to join my father in developing a program in plant pathology. I was in grade school. I recall his skillful participation in the faculty softball games, the Iowa State picnic outings, and the family associations of those early years. Later on, in high school athletics, I experienced another relationship with Lysle: he was the official at several of my school's football games.

In subsequent years, as I was pursuing my education leading to a career in plant pathology, Lysle fulfilled another role during my visits to Davis—that of advisor and teacher, although both in an informal rather than a formal way.

The continuation of our association seemed to be an act of destiny. When I joined the University of California as a young plant pathologist, Lysle and I became colleagues with common interests in diseases of seedlings, vegetable crops, and treatments of seeds with fungicides. Our official association culminated with our simultaneous chairmanships of the Departments of Plant Pathology, he at Davis and I at Riverside.

While our particular association is unique in many ways, it is not unusual in that Lysle has had and continues to have strong personal relationships with people both within and outside the field of plant pathology. His interest in the lives and careers of young people, his natural talent as a teacher, his creative activities in research, and his unselfish devotion to the service of people in agriculture throughout the world exemplify the breadth and quality of those few faculty members who are labeled outstanding by their colleagues and peers.

Lysle's contributions, not only to his professional science, but to the lives and accomplishments of his students, friends, associates, and colleagues, will continue as reminders of the greatness of this man.

This history will give the readers insight into the ways in which a person of destiny progresses through life, leaving indelible markers along that path for others to observe and to follow, if they are wise enough to do so.

I commend it to you with pride and respect.

J. B. KENDRICK, JR.
Vice President
Agriculture and University Services
University of California

INTRODUCTION

LYSLE LEACH AND I attended Kansas State Agricultural College (now known as Kansas State University). The last semester of his senior year coincided with the first semester of my freshman year. This was the fall of 1922.

Seven years later, I came to Davis as an assistant professor in agricultural engineering and found Lysle here in the Department of Plant Pathology. We had very little in common during the next ten years except in a social way in faculty club activities.

In 1938, the U.S. Beet Sugar Association made a grant of $80,000.00 to the Department of Agricultural Engineering to cover research on all phases of sugar beet production.

One of the problems recognized from the start was the multi-germ seed balls. When planted, they produced from one to several seedlings per seed ball. This made short handle hoe blocking and finger thinning imperative in order to obtain uniform distribution of single seedlings, an operation that required twenty to thirty man-hours per acre.

The first step was a literature search to learn what plant breeders had done toward producing mono (single) germ seed. While encouraging during the first few years, the work was terminated about 1912. According to Al Skuderna of the American Crystal Sugar Co., the breeders were up a blind alley.

During a meeting with the Sugar Beet Advisory Board in 1940, the statement was made that an attempt would be made to produce single germ seed balls mechanically. Herman Zitkowsky, president of the American Crystal Sugar Co., a member of the board, stated that the Germans had developed a mechanical process for reducing the number of germs in a seed ball. Correspondence with Dr. W. Knoll of the Institute of Land Machines at Halle, Germany, brought a courteous reply but no technical information. A machine developed at Davis in 1941 sheared seed balls into segments as they passed between a revolving emery wheel and a stationary shear bar.

An immediate problem was the treatment of the segmented seed for protection against seed-borne diseases. Dr. Leach was consulted. He was of the opinion that some work should be done in both the greenhouse and field to determine the proper dosage of fungicides. From then, until the project ended ten years later, he became an invaluable member of the research team.

By 1944, some 900,000 acres in the United States were planted with segmented seed with an estimated savings of nine million man-hours. About that time, Bion Tollman, agriculturist for the Utah Idaho Sugar Company, had demonstrated damage to seedlings produced from segmented seed. Lysle and I began the consideration of a less severe process other than shearing.

Lysle suggested grinding the seed down to some predetermined size. A process called decortication was developed. It used a carborundum stone on a vertical shaft with an adjustable neoprene pad for holding steel balls against the revolving stone for grinding off surplus cork. The resulting product was smoother, more uniform in size, better adapted to precision planting and contained a reasonable percentage of single germ units.

By 1948, decorticated seed had replaced segmented seed. It filled an important gap until Drs. V.F. and Helen Savisky, working with Dr. F.V. Owen, a Utah geneticist, developed monogerm seed. The first commercial release was in 1954.

Through a lifetime of research, Lysle became an international authority on sugar beet diseases and their control. His counsel was sought worldwide. He made several trips to South America and to Europe advising on disease control measures. This did not stop when he retired from the University.

Lysle did not let research overshadow his relationship with students. He was an excellent teacher. He was able and willing to develop and teach courses in plant patholology at three levels of instruction—non-degree, undergraduate and graduate. His experience as dean of students and faculty representative in the Far Western Athletic Conference gives an insight to student life and problems on the Davis campus that is not available from any other source. As I read through Lysle's memoirs, it particularly impressed me that we now have a picture of what student life was like during earlier decades of campus history.

Another important contribution this volume makes is the history of the development of plant pathology on both the Berkeley and Davis campuses. The increase in graduate students in plant pathology from one to fifty-seven for the period of 1933 to 1967 is astounding.

I feel privileged to have had an advance copy of Lysle's oral history. The late A.I. "Dick" Dickman and Jack Hills did a wonderful job of pulling out information concerning the life of a great man. I personally liked the way the story went together. His daughter Joann Larkey had a lot of fun putting the material in final form.

ROY BAINER
Dean Emeritus
School of Engineering
University of California, Davis

HISTORY OF THE INTERVIEWS

COMPLETION OF THIS memoir by Lysle D. Leach creates another milestone in the ongoing efforts of the Oral History Office of Shields Library at UC Davis to preserve the recalled experiences of persons who have made distinguished contributions to the development of the Davis campus and to California agriculture. In 1973 his name was among a group of emeritus professors at Davis who were selected by a faculty advisory committee as prospective memorists, principally because of his association with the Department of Plant Pathology at Davis, beginning in 1929. In addition to a career of teaching and research that brought him academic honors and an international reputation for his work on plant diseases, he had participated extensively in student affairs and intercollegiate athletic programs.

During the intervening years, Dr. Leach's active post-retirement research on seedling diseases, his natural reticence to call attention to his own achievements and university budget restrictions all combined to keep this oral history project on the back burner. Eventually, the charisma of Dick Dickman, head of the Oral History Office, a friend and occasional golfing buddy, provided enough convincing encouragement to get the tape recorder rolling. These two men had another common bond—an abiding interest in the history of the Davis campus.

Dickman volunteered to interview Dr. Leach about his early life and his long association with developments at Davis. Frederick J. (Jack) Hills, a former student, fellow plant pathologist and extension agronomist at Davis since 1951, agreed to conduct supplemental interviews regarding the scientific aspects of Leach's career. Having been involved in the oral history program at Davis on a part-time basis since its beginnings in 1970, I agreed to help with research and editing tasks, hoping to minimize my involvement in the interview process with the man who is my father. It should also be noted that in honor of Lysle Leach's eightieth birthday, on November 10, 1980, contributions to fund this oral history were made by members of the Leach family and a few longtime friends, professional colleagues and former students. Also, many letters received at this time contained personal recollections about past associations with the honoree that provided added insights to the prospective interviews. Despite his involvement in numerous other partially completed oral history projects and health problems, Dick Dickman found time to meet with Lysle and Jack Hills in early December 1980. Together they agreed on basic outlines of subjects to be covered in their respective interviews.

Subsequently, a series of four Leach/Dickman taping sessions were held in the Oral History Office, a fourth-floor book-lined corner of the Rare Books Section of the Shields Library's Department of Special Collections. These occurred on December 22, 1980 and on January 5, 12 and 19, 1981. Conversations lasting about an hour and a half each were recorded on a reel-to-reel machine. Four more similar interviews were conducted by Jack Hills at the Leach residence, 234 Rice Lane in Davis.

Both interviewers quickly found that their narrator came to these taping sessions well prepared, with detailed notes on the subjects he knew would be discussed. The organized, methodical approach of a trained scientist might seem to some readers as lacking in conversational tone. However, those who know Lysle Leach personally can attest to the fact that even in spontaneous conversation he speaks in clear, concise and well constructed sentences, saying exactly what he means and not hesitating to discuss controversial issues.

During April and May of 1981, the tapes were ably transcribed by Candy Peel and Dickman began the initial editing. Then came a protracted pause in this and other oral history projects. Dick's cancer was recurring and progressive. Yet, he continued the work he loved so much until his uphill battle was lost on August 26, 1981. His death was a personal tragedy to all who knew him and a major loss to the University. This volume has been dedicated in his memory.

I completed the initial editing and sequential arrangement of the manuscript in the fall of 1982. The Leach conversations with Jack Hills, to be found in Chapters X through XX, deal principally with the former's research and foreign assignments. These required a minimum of editing by both parties. Jack Hill's own history of these interviews follows. The Leach/Dickman interviews were edited to insure continuity and to eliminate duplication between this two-part memoir.

The narrator, taking his role as the final editor seriously, wished to make some additions and deletions to ensure clarity and accuracy. After a careful review of the entire transcript, we mutually agreed that he should be asked to tape-record some additional comments on a few subjects, principally those dealing with the development of the UC Department of Plant Pathology since its beginnings in 1903, his involvement with student affairs, growth on the Davis campus and family activities. Consequently, I posed a new series of questions which he answered in amazing detail, without prepared notes, pausing only occasionally to look up something in the transcripts of previous interviews that were piled between us on the kitchen table in the Leach home. Transcriptions of these supplemental interviews appear for the most part in Chapters VI through IX, XXI and XXII. For the sake of continuity, they were inserted into the text as though they were part of the Dickman interviews.

A postscript to the memoir was added following the University's dedication of the UCD graduate student residence complex as Lysle Leach Hall at ceremonies held in Freeborn Hall on October 22, 1982. The narrator, who still spends most days at his office or in the greenhouse, delayed his final reading of the text only long enough to write up his most recent research experiments and drive to Phoenix, Arizona in February 1983, where he presented his findings at a national meeting of sugar beet technologists.

My involvement in this oral history project has been a rewarding experience, even a privilege, as I gained new knowledge and insights into the varied career of my father. Never before had I heard the story of the Great Dormitory Water Fight of 1954, or seen some of the old family photographs that were unearthed to illustrate this volume. Lysle's eagerness to write into the record the statewide significance of Professor Ralph E. Smith's early plant disease work at the University of California and the subsequent contributions of his fellow plant pathologists on the Davis campus attests to the thoroughness and attention to detail with which he approached this project. We are both indebted to Erna Thompson, longtime departmental administrative assistant, who provided invaluable resources from her files, offered encouragement at every step and assisted with final proof-reading. Logistical support and publication details were ably handled by Don Kunitz, director of Special Collections, and Lawrence Morgan of the Oral History Office.

Appreciation is also expressed to Vice President James B. Kendrick, Jr., for his preface to the Leach memoir and to Roy Bainer, Dean Emeritus of the College of Engineering at UC Davis, for his willingness to write a comprehensive introduction.

Joann Leach Larkey *for*
Avrom I. Dickman
July 5, 1983

IT MAY BE of interest to those who will read this memoir of Lysle D. Leach to know that I willingly became involved as a volunteer interviewer when the project was proposed to me by Mr. A.I. Dickman, head of the Oral History Office of Shields Library of the University of California at Davis. Lysle, Dick Dickman and I agreed on my conducting a series of interviews pertaining to Lysle's many years of teaching and research in the University's Department of Plant Pathology.

In preparation for these tape-recorded interviews, Lysle and I reviewed a detailed set of notes he had made concerning his professional career at Davis and decided on a sequence for discussion. Then we just started talking. During four recording sessions, the first on January 20, 1981, in the Oral History Office and the rest in the Leach's living room on February 2nd and 3rd and March 26, 1981, Lysle recounted the circumstances of many milestones in his academic career with a minimum of prompting from me. His responses and observations reflect his ability to organize subjects and present them logically and precisely. The sometimes informality of conversations about technical subjects can be explained by our longstanding acquaintance and mutual interest in plant science.

My association with Lysle goes back to early 1942 when I first went to work for the Spreckels Sugar Company in the Sacramento-Woodland area of California. I helped apply lime to plots in the Sacramento Delta, when Lysle was trying this to control Aphanomyces seedling disease of sugar beets. I also helped with some of the early field trials to test segmented seed, which is discussed in detail in Chapter XIV. Soon after this I went off with the U.S. Marines, but returned to Spreckels and my association with Lysle.

In 1949 I decided I needed more detailed training in plant pathology and started taking courses at UC Davis and doing research on sugar beet seedling diseases under Lysle's direction. After obtaining a master's degree in plant pathology in 1951, I got my present job with the University as an extension agronomist and have worked with Lysle ever since, including during a sabbatical and an additional extended leave to get a Ph.D. degree in 1961—again with Lysle as my major professor.

Lysle has always been known for his meticulous research on problems important to the farmers of California. The amazing thing to me is that even though with advancing age he has slowed down a little physically, he is still as mentally alert and as sharp as ever.

It has been a real pleasure for me to participate in these interviews. But most of all, it has been my good fortune to have had Lysle Leach for a teacher and friend for all these years.

F.J. Hills
June 10, 1983

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FAMILY HERITAGE, CHILDHOOD RECOLLECTIONS

The interviewer is A. I. Dickman.

Lysle, I understand that you've been associated with the Department of Plant Pathology at the University of California since 1928 and have been located at Davis for the past fifty-two years.

Yes, it's been that long. On February 1, 1929 my wife Jane and I moved from Berkeley to Davis with all our possessions in two cars: a Model-T Ford the University wanted us to transfer to Davis, and a second-hand Buick Coupe we had purchased a few months previously. We have lived in Davis ever since.

Because of that fact, your longtime observations on the growth and development of the Davis campus should be of particular interest to others. But first, let's begin with some recollections of your early childhood and your family heritage.

My father was English and my mother was German, so I am half-and-half.

Your father was a native of England?

No, but his parents, Isaac and Louisa Clair Leach, were born and married in England. They came to the U.S. in the early 1850s and settled on a farm near Chagrin Falls, Ohio, where my father and his four brothers were all born. My father, the fourth child, was born on March 31, 1861.

What was his name?

John Wesley Leach, which indicates a good Methodist background. In the late 1880s, my father left Ohio and moved west to Lincoln, Nebraska. The other four sons remained in the Cleveland area essentially all of their lives. My father was the only one who left the area and moved west. I know that he attended the medical school at the University of Nebraska for a period, but did not complete his medical training. I was told by my mother that during his training he was assigned to sit up with a child who had a very severe case of whooping cough. During the night the child strangled and died, and this seemed to have affected my father so that he decided he did not want to continue with medical training.

He purchased a cottage grocery store in Lincoln and operated it for a number of years. It was in Lincoln that he met my mother, and they were married in [May 3] 1894. My mother was born March 18, 1870 at Clatonia, Nebraska, about thirty miles south of Lincoln. According to the records in the German Methodist Church, she was one of the first children born in Clatonia township [Gage County].

What was her name?

Her name was Emma Louise Drewing and she was the daughter of Louis and Sara Bloom Drewing. My mother, after she moved to Lincoln, attended business school, and following her marriage to my father she served as bookkeeper and clerk at their store. My father had always been interested in purchasing farm land with the idea of improving the property for resale. His first such venture was in 1898, when he sold the grocery store in Lincoln and purchased 160 acres of land in Holt County in north-central Nebraska. He purchased this land from a George Meisner, who had homesteaded the farm in 1890. I still have the patent, or homestead certificate signed by or for Benjamin Harrison. My father, mother and their two-year-old son, Leonard [b. October 4, 1896] moved to this farm, 14 miles north of Stuart and Atkinson. It was here that I was born on November 10, 1900. There was a post office by the name of Ingels about five miles from our farm. But this has since been abandoned. So, on my birth certificate, my birthplace is listed as Stuart.

Were you born at home?

It is my understanding that I was born at home with a midwife, but my daughter remembers my mother telling her that my father delivered both my brother and me.

Lysle, what do you remember about the first years of your life?

My first recollections would be of the routine farming operations. I remember quite early that during the hay harvest my brother encouraged me to feed some of the cut hay to a stationary mower while he was revolving the Pitman bar. I became a little too enthusiastic about this and cut halfway through my thumb. I still have a scar from that. I also remember there was a prairie fire which was pretty frightening, that burned pretty close to our home. I remember a large number of men fighting the front margin of this fire by using wet burlap sacks, and the job of the rest of us was to wet these sacks when they were to be changed. Fortunately, they stopped the fire before it crossed the road and there was no damage on our property.

At one time, my brother went out to drive in the cattle and came in holding an animal on his back, saying "See what I got." And it turned out he had a badger he had hit over the head with a stick. But by the time he got home the badger was recovering and was looking around preparatory to taking some action. The badger had to be put away quickly.

At about four or five years of age, I went to school with my brother even though I was too young, and I got quite early training in reading and simple arithmetic. I recall one of these trips when we stopped along the creek on the way home and located a very large mud turtle. My brother threw it out of the creek and it was my job to hold it down while he hunted for something that he could use as a weapon. We managed to kill the turtle and brought it home. We had heard about turtle soup so we insisted that our mother prepare turtle soup from our game.

Where was your school located?

The school was a little over a mile directly west from our farmhouse.

Describe it, please.

It was a typical one-room school and there were, as I recall, about eight or ten students in grades all the way from the first to perhaps the sixth or seventh. There was, of course, a single teacher and she went from one student to another and covered all subjects. There were just simple wooden benches and tables.

How did you get there?

We walked. I don't recall going during severe weather.

Did you run into any tornadoes?

Yes, Nebraska was well-known as cyclone country, and every home had a dugout that doubled as a root storage cellar and as a cyclone shelter in the case of an emergency. At first we had a small house with only a few rooms. Later, my father, with the assistance of neighbors, built an addition to the house. And at one time a tornado came through and moved part of the house. I know that it had to be jacked up and restored on the foundation. That's the only storm of that kind that I can recall.

Now, you went to school with Leonard. Did you also go to this school on your own?

No. It was really preschool training. In September 1906, when we moved to Fremont, we were tested. I was placed in the first grade, while my brother was placed in the third grade.

Why did you move?

Well, I think my father had decided that northern Nebraska probably wasn't the place to spend the rest of his life, so he went to Fremont, purchased a grocery store and then returned and sold the farm.

What kind of school was the Fremont school?

Fremont was a town of around 12,000 population and, as I recall, had very good schools. So I think we were fortunate to have most of our early education there. We lived in three different houses in Fremont, and I attended three different schools before entering what now would be called junior high school. It was one of the larger towns, about fifth or sixth in Nebraska. In addition to good schools, it had a lot of churches, nice parks, and some very nice residential areas. So it appeared to be quite a prosperous community. It was located on the north side of the Platte River and the country around there was very flat, so that in the spring when the ice melted, the river commonly flooded—sometimes right into the southern part of the city.

Did you ever have to evacuate?

No, the areas where we lived were more to the north and east and I don't think they were ever flooded.

Let's talk about some of the things that you did as a youngster. How about chores? Did you earn any money?

While we were on the farm we were doing chores, but I don't remember that we were doing it for money. But soon after we moved to Fremont there were quite a number of things that I did. Quite early I sold magazines such as *The Saturday Evening Post,* one that was called *Grit,* which no longer exists, and several others. At a fairly early age I had a paper route delivering the *Fremont Evening Tribune* for a number of years. This is the way I earned my first bicycle and, of course, that was a necessary tool of the trade.

In about 1910 my brother and I took on the summer job of delivering groceries for our father's store. Each of us had a horse and a delivery wagon and we took care of that chore during the summer for pay. My main recollection is that my dog, Joe, was my constant companion as I rode around town delivering groceries to various customers.

You've shown me a picture of the Leach grocery store. Could you describe the store? You were a young boy then.

Well, at this period of time there were no supermarkets and this was a typical grocery store, very small in size, located on a corner in Fremont about two or three blocks from the railroad depot.

Do you remember the corner?

Well, I think it was Third and Main Street. This was a store for groceries and general provisions as most grocery stores were at that time. They carried mainly fresh and canned goods, with some household supplies.

How long did your parents operate that store?

They operated it from about 1906 to about 1914, so it would be around eight years.

Did you and your brother get paid for delivering groceries, or was that your contribution to the family business?

No, we were paid for this. I don't remember how much, but each of us delivered in different parts of town and were paid a regular wage for this during the summer months.

You mentioned one time that you and your brother had a little business on the side, in the back of the grocery store.

Yes, there was a small shed directly behind the grocery store and I assume that it was my brother who thought up the idea that if we would clean up that shed, we could have a little business of our own. So we cleaned it up and made a window for people who would walk by and we made arrangements with my father to buy produce from him at wholesale cost. This was primarily oranges and bananas and apples—fruit of this kind. And then we set up our own little shop back there and sold to people who would come along the street. We learned rather quickly that our business improved considerably if we reduced our prices a little, so we were selling this produce at a few cents apiece less than my father was charging in the store.

How did he feel about this?

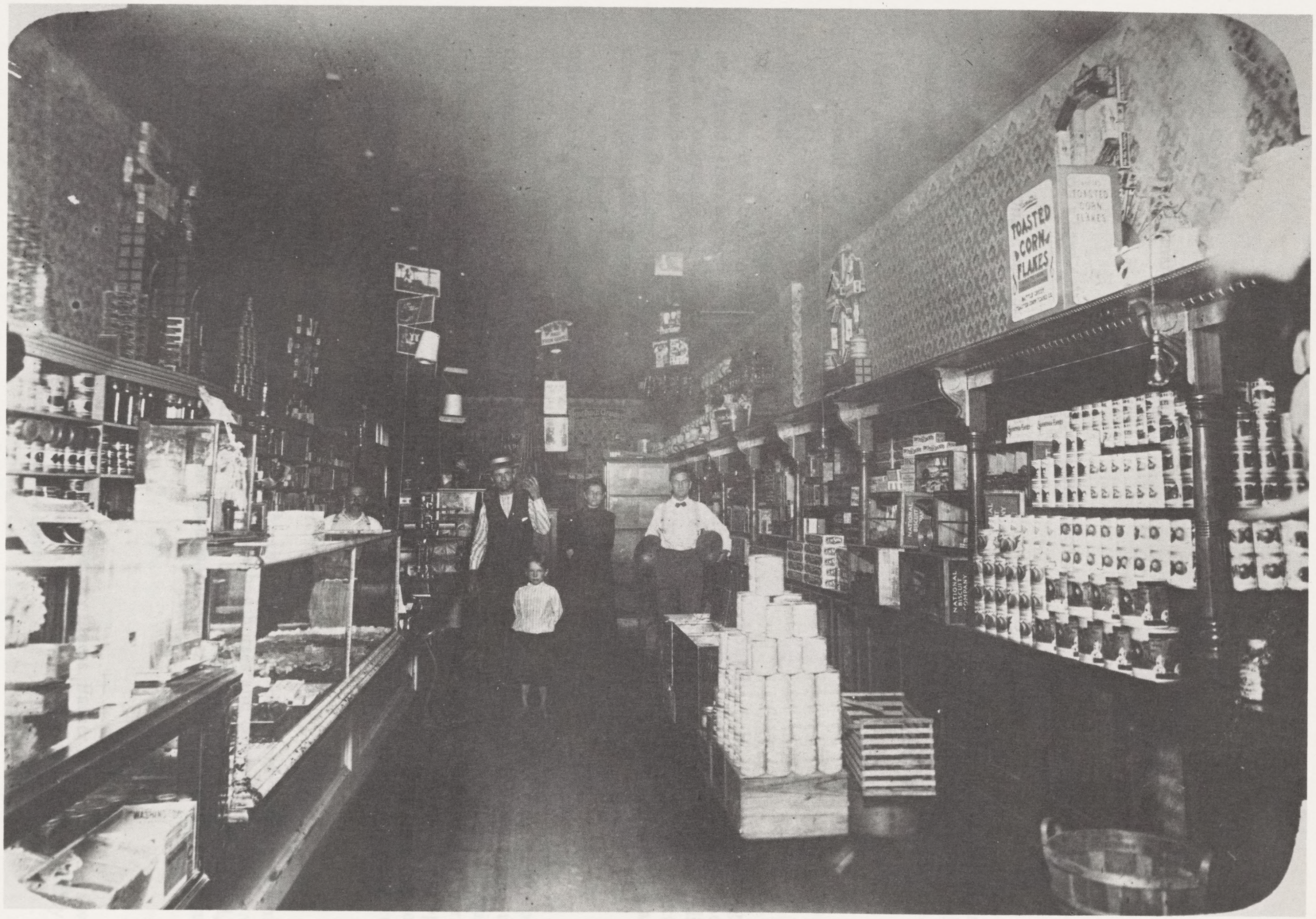
Well, we were cutting in to his business a little, and I don't think he thought too kindly of it. At least, this didn't go on for too long before either we got interested in something else or possibly he discontinued it himself. I'm not sure.

How close were you to your parents?

I think you would say that we were quite a close-knit family, but both my father and mother were quite busy with their own activities. So a good part of the time that my brother and I were at home doing our own things we were not under close supervision. We probably did not see our parents as regularly or continuously as many children do. Both of them were involved in the store. My mother always kept the books and did a great deal of the buying, while my father was busy taking care of other aspects of the store.

Left: Leonard L. and Lysle D. Leach, Fremont, Nebraska (c. 1906).

Above: Lysle Leach and his dog Joe in the Leach grocery wagon (c. 1911).



The Leach family grocery store in Fremont, Nebraska (c. 1908). *Left to right:* Clerks Hansen and (unknown), Lysle with parents Emma and John Leach.

Who disciplined you?

I would say primarily my mother.

How?

Well, I don't have any very strong recollections of this.

Did you and Leonard get along well?

I would say in general we did. But with four years between us it was sometimes a little difficult. He obviously had the upper hand. We had the same friends at times and were together quite a little. But I was always the younger brother.

Was it a religious family?

I would say not very religious. We did attend church regularly.

Methodist?

Yes. As we lived in different parts of Fremont, I attended Sunday School more according to my friends than according to the religion. I remember at one time I attended the Sunday School at the Congregational Church for a number of years, and other times went to the Methodist. I remember that I earned prizes, or certificates for attendance, for reciting the "golden texts" and for memorizing the names of the books of the Bible. I would say that we did not have any close religious affiliation.

Were you a Boy Scout? Did you belong to the YMCA?

I belonged to the YMCA. I'm not sure whether they had an organized Boy Scout troop at that time in Fremont, but the YMCA was quite a center of activity, particularly during the summer months. And it was there that I learned to swim and take on many other activities. I remember particularly that at one time they organized a series of visits to different industries in town where we went to factories and to various commercial developments and the people explained to us how the operations were conducted, and gave us an insight into what happens in business and industry. I remember being interested in the ripening of bananas and other fruit that was shipped in from long distances.

Did the family have any eating habits that were particularly their own?

It was pretty much, I think, according to the times. We very commonly went on picnics on Sunday and went for rides in the country since we had horses and conveyance. And so we saw a good deal of the countryside and my mother being German set, what I would call, a fairly substantial table—lot of concentration on desserts. But otherwise I don't recall any unusual food or eating habits.

How about the general health of the family?

It was very good. My mother never seemed to have any illness. Father in the early days was quite healthy. Later, when around age 50, he did have a fairly serious operation in Kansas City. I know that the situation was fairly critical for a few days.

What was it for?

I believe it was related to his gall bladder.

Wasn't Fremont along the famous Lincoln Highway?

Yes, it was directly on the highway. In fact, when I was attending the Central School in the seventh grade, the new Lincoln Highway went directly past our school. To celebrate, they had a ceremony, a dedication of the highway at the time it opened and we were all encouraged to bring our dimes to contribute to the establishment of the markers for the Lincoln Highway, the first transcontinental highway. So I was interested when we came to Davis to find several markers at that time in Davis and the last one I can recall was located on the northwest corner of Second and B streets. And I think our daughter Joann was in part instrumental in removing and preserving it for the future.

Did you continue to live in Fremont through high school?

No, while I was in the eighth grade my father decided to sell the grocery business as he had previously purchased a farm near Central City, about 100 miles west of Fremont, also on the Lincoln Highway. We moved in the very early Spring of 1914—probably February—to this farm and I completed the eighth grade, in another one-room schoolhouse. I got my eighth grade certificate from this rural school.

To qualify for this certificate, we all had to take a uniform county examination and I have two recollections from this. One of my favorite subjects was oral arithmetic and I managed to make a grade of 100 in the examination for oral arithmetic.

What's oral arithmetic? Sums in your head?

That's right. Without using pencil and paper. They would read off the problem and you had to come up with the answer.

Do you still have that knack?

I think I do because I do sums in my head, make approximations whether it's at the grocery store or working out tax reports. I usually solve the problem as close as I can before putting it on paper.

So your father then went back to farming?

He did farm, but, looking back I would say that his interest was more in purchasing farm land and developing it and then selling it for a profit rather than operating a farm continuously. For example, after moving to Fremont, he bought a farm along the Platte River which was a very good farm except that it was subject to overflow and after a few years he sold it. He bought a much larger block of land in Kimball County, in the far west of Nebraska, which was less expensive grazing land. I understand it's now the site of a prosperous oil field. And he bought another farm along the Missouri River which he sold when it appeared that the river might cut into one corner of the farm. So while he owned a number of farms, he usually would retain them only for a few years and sell them whenever he thought it was to his advantage.

But we did live on the farm at Central City from February to November of 1914, and in the meantime we improved the fences and painted the buildings and did a lot of repair work. Then he sold the farm and we moved to southern Kansas. But while at Central City, my brother and I entered high school and attended from September until November. We were four miles from high school, so we drove a horse and buggy in each morning and back each evening. That didn't leave us very much time for participating in school activities by the time we made the round trip each day and did our chores morning and evening.

You say that shortly thereafter you moved to Kansas?

Yes, in November 1914 he sold the farm at Central City and we moved to a farm just south of Winfield, Kansas. My brother rode with the farm stock and our belongings in the freight cars and the rest of us travelled in passenger cars.

I remember particularly well our arrival at the small railroad station called Hackney, a few miles out of Winfield, because when we arrived, there was a half to three-quarters of a mile line of horse-drawn farm wagons lined up at the grain elevator. This was not a welcoming committee for us but it was the first day that the price of wheat had reached $1 per bushel and many of the farmers had been holding their wheat in anticipation because this was shortly after the start of World War I in Europe.

II

A KANSAS EDUCATION

Winfield, Kansas, then, was your high school?

Yes. All of my high school except the first two months was at Winfield. Winfield was a town of about 10,000. Not an industrial town, but a very fine school town, with good local business and many churches. In fact, while I was in high school, the city received an award as the best town in the state of Kansas for raising of children.

Talk a little about your courses, your grades, your study habits.

I took, as most everyone did at that time, the regular college preparatory courses: English, mathematics, science, language and history. I was particularly interested in mathematics and science and I seem to have done fairly well in most subjects without too much effort so I always assumed that I would go on to college. My parents gave that encouragement to both my brother and me.

Your brother went on to college, did he?

He went to the University of Nebraska two years before I finished high school and continued there until he enlisted in the U. S. Marines in 1917.

Were you too young for military service?

Yes. We didn't enter World War I until April 6, 1917, so I was only sixteen when we entered the war. In high school it was assumed that all of us would be in the army before it was over, so we were given, during my senior year, an intensive training in conversational French. We had a very good instructor and since this was an extra subject, I was very fortunate to get a good training in that during high school. At the time I graduated from high school, I was seventeen, and still too young for military service.

Did you date when you were in high school?

Very little. I was younger than most of the people in my classes and I was smaller; I would say less mature. As a result, my social life was not very active. More time was spent with my friends and other activities.

Did you partake in athletics?

I was very interested in them but they did not have a football team in that high school because there'd been a fatality a few years earlier. They had a very strong basketball program. I was too small to play on the varsity so I played on what was called the midget or the peanut squad. I was not good enough or big enough to make varsity in basketball.

My main activity in that area was in gymnastics. I took quite an interest in physical education and in gymnastic activities. Because of my small size, I always got the position of top man on all of the pyramids. I spent a good deal of time in gymnastic activities.

How many were in your graduating class at high school?

About 125. There were over 500 in high school at the time.

Just as a guess, how many of these went to college?

This would be a pure guess but I would think that probably forty percent. It was quite high, I think, for that period, and one of the reasons is that there was a Methodist college, Southwestern College, located in Winfield. It was very common for graduates of Winfield High School to go on to Southwestern College if they did not go away to Kansas University or what was then Kansas State Agricultural College.

Had that idea ever occurred to you?

No, I never considered that. I think usually you're influenced in your college decisions by some individual and in my case it was Frank Root, the basketball and gymnastics coach who had graduated from and had been a prominent athlete at Kansas State Agricultural College, now Kansas State University. So I and most of my close friends went to Manhattan and had planned to do so probably during the last year and half or two years of high school.

Did you have a clear idea as to what you wanted to study?

I had an idea that it would be something in the agricultural line, but I did not have any very clear idea of just what it was going to be. Actually, when I arrived at Manhattan, I registered in Engineering because in that way I could get courses such as Surveying and Trigonometry, which I was planning to use if I entered Officers' Training.

You were in the military during World War I?

Yes, briefly. I went away to college in September 1918 and the last registration for the draft was on September 12. It turned out that most of my friends with whom I had gone to college were just a little older, so all of them were registering at that time. My birthday wasn't until the tenth of November so I was the one who was left out. I finally decided that I would register at the same time they did anyway. I conveniently changed my birthdate from November 10 to September 10 and I registered on September 12. We were then inducted into the Students Army Training Corps on October 1, which was an actual induction into the army. We lived in barracks and had regular army officers, but it was located at the university [at Manhattan, Kansas]. We were each given three courses during the day and the rest of the time was in regular military activities.

From October 1 to December 12, I was in the army and preparing to attend an officers' training school. The armistice was signed on November 11 and all the military units were called out for a march through town. But in the United States there was a false report that an armistice was signed the day before. So we marched for the false report and then again on the eleventh which was the official Armistice Day. We continued our training and course work until the twelth of December when the SATC was discontinued and we were all mustered out. This was in part because of the flu epidemic that had been very serious that Fall and all schools at that time were closed until after the Christmas holidays.

Did the flu epidemic hit you or any member of your family?

No, my brother at that time was in the Marines stationed in Haiti and my parents were spending the winter in Brownsville, Texas. I did not contract the flu, but in our squad of eight, six of them did have the flu and some were quite seriously ill.

You were then able to continue your college studies?

Yes. In the second semester I transferred to the College of Agriculture and was registered in Animal Husbandry. But I was fortunate, I think, that at Manhattan they permitted a rather wide choice of courses. It was not as restricted as it is in most agricultural colleges, including the one at Davis. We had somewhat more choice in a wide range of subjects and that worked to my distinct advantage in my final selection of a field of studies. My first real introduction to the subject of plant pathology was when, at the beginning of my senior year, I took a course in plant pathology under Professor Henley Haymaker, who was a very good teacher and very personable. He made the subject very interesting to me. I was particularly attracted by the very complex relationships that existed between the pathogens and their host plants, and the very interesting cycles of development that were completely foreign to anything that I had ever known before.

In addition to Dr. Haymaker, what other professors influenced your life?

Well, there was a Professor [R. I.] Throckmorton, professor of soils who had graduated from Pennsylvania State University. He was an excellent teacher and later became Dean of the College of Agriculture there and quite prominent in national agricultural circles. He used to spend quite a little time with us as undergraduate students talking about subjects other than soil science. I remember that he was the first to introduce me to the concept of the honor system, explaining to us how this had operated when he was an undergraduate and how important he thought it was for college students to learn to accept responsibilities and to administer their own examinations. This was very valuable to me when later I came to Davis where at that time there was a very strong honor system and I was involved in the faculty aspects of it.

Did you begin to date in college?

Yes, when I first came there I'd made no advance arrangements for housing. There were no residence halls at that time, so the housing office arranged a room for me in a rooming house. I was put with a student with whom I had very few things in common, and it did not look like it was going to be a very good arrangement. But then I was rushed by one of the fraternities in which some of my friends from home were members, and I was pledged to this fraternity.

Which one?

It was at that time a local called the Shamrock Fraternity but it was petitioning Kappa Sigma, one of the strong national fraternities. My first year was our year of petition, and we were installed as the Gamma Chi chapter of Kappa Sigma in June 1919, at the end of my first year of college. Here I moved in with a group and, looking back, I can see that I had a real need for a group of close friends with whom I could live and exchange information and experiences. This also gave me an opportunity to learn to dance, which I had not learned in high school, and to participate in social activities. So, I dated, not too frequently, but on all of the important occasions.

How about extracurricular activities?

I was short of money and the first thing I needed was a job. I held a number of jobs that paid by the hour. One time I delivered for a cleaning plant and I had a Model-T Ford that was very resistant to starting during the winter months.

Did you crank it?

You cranked it—there was no other way. You had to crank it and choke it and get it started the best you could. And so I delivered cleaning and laundry for my outside job.

You had your own car?

No, it was furnished by the cleaning plant. Later I worked at a produce company that purchased eggs and chickens, particularly eggs, and then packaged them for shipping to Kansas City and Chicago.

I was quite interested in athletics and was very active in the intramural program. I was the manager of the fraternity basketball team and participated in all the intramural sports. But I was not good enough to make a varsity letter in any sport, although I tried several of them.

Did you get some money from home?

Yes. I'd earned and saved several hundred dollars when I started and at one point when I was getting low I know my father furnished me $700 which was put on a note. But later it was settled by giving the same amount of money to my brother. So that $700 I did not have to pay back. Everything else, I earned or borrowed and paid back.

When did you begin to consider graduate school?

Not until after I'd graduated. After three years of college I ran out of money and I didn't see any prospects for financing the fourth year, when I learned that it was possible to get a temporary certificate to teach in high school for one year with only three years of college. Through the employment agency I made some contacts and located a high school about fifty miles from Manhattan that had such a position open. I applied and was employed to teach in that school even though I had not finished college.

Where was this school?

Eskridge, Kansas Rural High School. I was only twenty years old and I was hired to teach a course in agriculture and one in biology. But on my arrival at the high school, they informed me that they did not have anyone to teach the senior physics class and since I had a course in physics on my transcript, would I teach the senior physics class too.

In addition?

Yes, in addition; everything always was in addition. Physics was not my favorite subject and it required much preparation, so I had to spend a great many hours at night with Hawkins' Electrical Guide and with other physics books in order to keep ahead of this class. To make it a little more difficult, I found that among this group of seniors were four who were older than I was. That I kept as a secret.

What were you paid?

The first job paid $165 a month for nine months and that was considered a pretty good salary. I roomed with a man by the name of Mr. [Lawrence] Shaw, who was the coach of all sports. Because I was interested, I immediately became involved in the athletic picture and I ended up as the assistant coach handling the freshman or reserve teams. This was something I enjoyed very much as it gave me a chance to be closely associated with the program.

What happened at the end of that year of high school teaching?

At the end of the year I found that I could complete my senior year by returning to summer school and fall semester and thus graduate in three and a half years, plus the summer school. I returned to Manhattan that summer, and took three courses and still had to complete twenty units in the fall semester to graduate in January.

And then what happened?

It was during this time that I took the course in plant pathology and this was the first course that I can recall that really attracted my interest. I wrote a term paper on the subject of "Downy Mildew of Grape," and I was fascinated by the information—how people had learned so much about the relationships and the way they had discovered the life cycles and worked for the control. This became a very strong attraction to me.

B.S. DEGREE, KANSAS STATE COLLEGE

But at that time my first job was to finish my degree, which I did during the fall semester ending in January 1923. Then I had to start looking for a job. It happened that one of my high school friends, Paul Tharp, was teaching algebra in the junior high school in Wellington, Kansas. He wanted to go to Oklahoma University to take petroleum geology so he made an agreement with the school board, that if he would find someone who would take his place as a teacher, they would release him from his contract so he could return to college. He contacted me and we arranged that I would take over his work for the spring semester. I taught several classes of algebra and was active in athletics because my roommate, again, was the high school coach. I coached his junior varsity basketball team and participated in general in the athletic activities. At the same time I played on the American Legion basketball team. We played other Legion clubs and some of the smaller colleges including Fairmount College, which now is Wichita University. We also played an Indian school located in northern Oklahoma.

USDA BARBERRY ERADICATION

Early in the spring of 1923, I learned that there were some jobs with the USDA on barberry eradication. I decided that I'd like to get more experience so I applied for a job in Iowa. But by the time school was out in spring, I'd heard nothing more about it and so I went to work in the wheat fields in northern Oklahoma, where each summer for the last three years I'd been working in the harvest fields. Then late in June, I received a phone call saying that they had an opening in the barberry eradication program and that I could have the job if I would notify them immediately and would arrive at Ames, Iowa, by the first of July.

Now this is USDA?

Yes. I decided that the USDA program would be more interesting than harvesting wheat, so left as quickly as I could, took the train to Iowa and arrived in Ames in time to go through their indoctrination program and be ready to start working on the barberry eradication on the first of July [1923].

Why did the USDA want to eradicate barberry bushes?

This is one of the subjects in which I had been very interested during the course of plant pathology. The black stem rust of wheat and other cereals has a very complex life cycle with a number of spore stages. The one we are most interested in is the red spore stage that develops on the wheat or oats in successive generations and brings about the destruction of the cereals. During World War I there was a strong campaign to "Win the War With Wheat." This campaign was handicapped severely because this black stem rust occurred in very severe form throughout the prairie states. Production was far below what they had hoped for. As a result, the government had set up a program of control. This control was based on the fact that the black stem rust spends only part of its life cycle on wheat, then produces an overwintering spore stage, and in the spring delicate spores are produced which are wind-blown on to an alternate host, the common barberry, the only host for this particular spore stage. Then another stage of the rust develops on the barberry leaves and spores are wind-blown to wheat or oat fields, initiating another cycle of rust epidemic on the cereals.

In many of the northcentral states, if you remove all of the barberry from a given area, the rust cannot then get started on the cereal plants because the spore stages on the wheat, the red spore stages, could not overwinter and the black spore stages would not initiate the infection on the wheat. So they developed this barberry eradication program in which they employed college students or recent graduates to travel through the rural areas and to locate all the bushes of common barberry and to get permission from the property owners to remove the barberry plants. The theory being that if you removed all of the barberries from the given area, the rust would not be started there the next spring.

This job provided my first contact with graduate studies in plant pathology. I was associated with people who were in graduate work, or who were thinking about it, and I began to think quite seriously about the possibility of returning to school and taking graduate work in plant pathology.

Did you start graduate school in 1923?

No. Not for two more years.

HIGH SCHOOL ATHLETIC COACH, ESKRIDGE, KANSAS

I'd been invited to return to Eskridge High School to teach and to serve as head coach of football, basketball, and track. So at the end of the summer of working on barberry eradication I returned immediately to Eskridge and started football practice. When the football team turned out I found that only three of the previous year's team members were back, but the good news was that nearly all of the good prospects had been on the freshman team which I had coached two years before. The first year of head coaching was really a building year, but the prospects of having all of them back for the next year made it really worthwhile. We managed to win five games, lose three, and tie two. In basketball, we fielded a tall but rather inexperienced team that improved considerably during the season. The team was strengthened immeasurably by a transfer who proved to be an excellent shooter. He [Faris Edgar] was our leading scorer for two years.

Did you have any black students?

There were only two black families in this community and two of the boys played on the football team, but I think none in basketball.

COACH LYLE LEACH

In the past many coaches have come and gone in Eskridge Rural High School, but none has been better liked than Coach Lyle Leach.

From the beginning of school until the end of the football season, he worked hard, and the football team when at its best form, had no difficulty in proving itself among one of the strongest teams in the state.

He coached a basketball team which won the county tournament and the St. Mary's invitation tournament.

Besides excelling in football and basketball coaching, Coach Leach is also a good physical director and track coach.

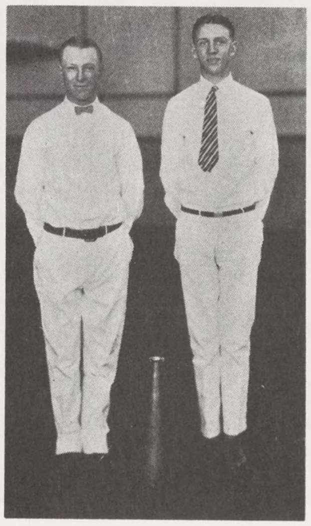
In addition to being a good coach and athlete, he is a good sport and a gentleman, and strives to teach his men to be the same. He taught the student body how to take, with a smile, defeat as well as victory.

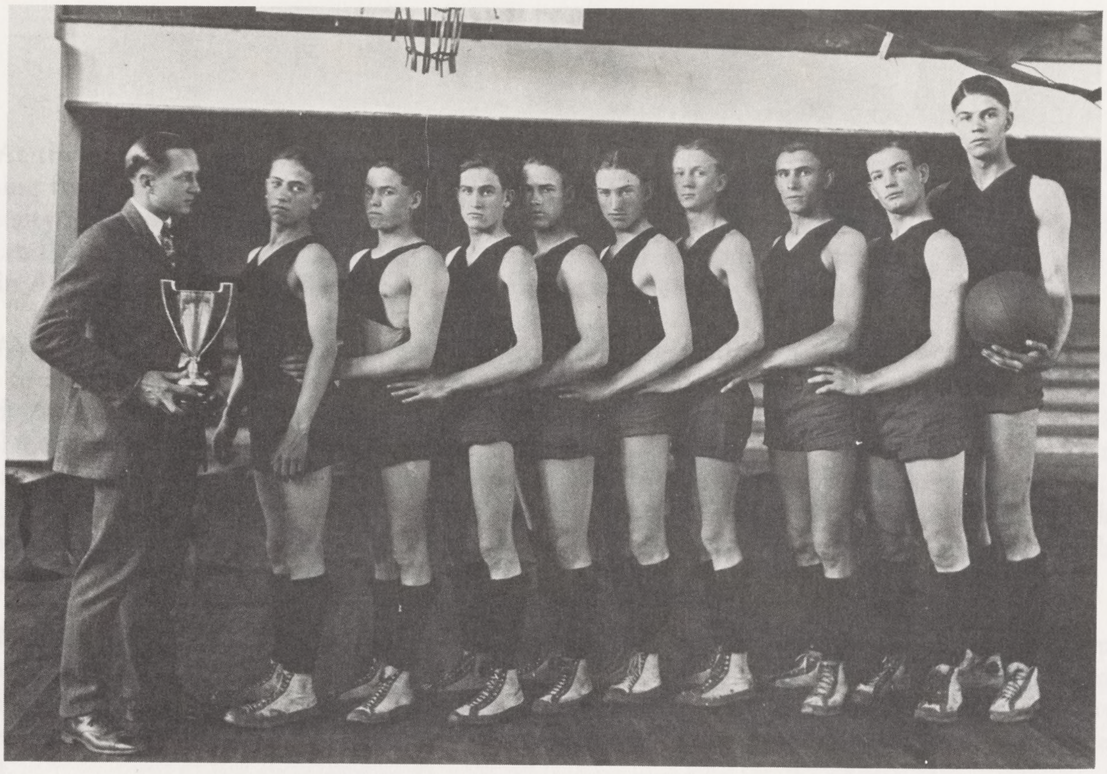
Every man who fought for E. R. H. S. under Coach Leach holds him in the highest esteem and will always remember "Leachy" and his coaching.

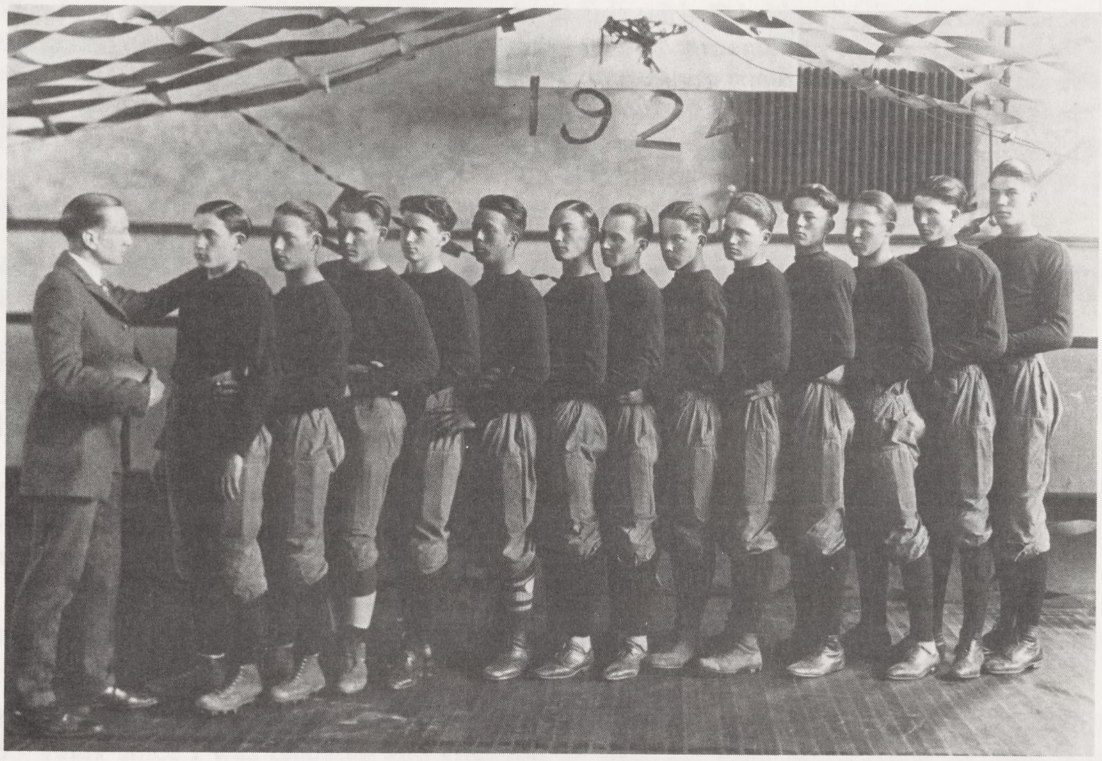
CHEER LEADERS

Our cheer leaders, Faris Edgar and Bumps Peoples, were always on the job. They organized the boys into a pep club, known as the Red Caps, for the county basketball tournament. They marched into the gym in a body and kept things going from the time they entered until the games were over.

Bumps' instantaneous humor never failed him in the most embarrassing situations. Faris found he could serve his school on the basketball court and on the track field, so he did not always lead the school in its cheering.







Above: 1924-1925 Eskridge basketball team, champions of the County League.
Below: 1924 Eskridge football team, also champions of the County League.

At the end of the school year, I signed to return for another year and immediately left on a fishing trip with two of my friends to the lakes of northern Wisconsin. We then drove to the University of Illinois at Champaign-Urbana to attend part of a coaching school. Football coaching was handled by [Robert C.] Zuppke, a famous name in midwestern coaching. By July 1, I was back in Ames [Iowa] for another season of barberry eradication with the USDA.

At this time I began to think seriously of graduate studies in plant pathology, and made plans to return to Ames after one more year of teaching and coaching. Upon my return to Eskridge in the fall of 1924, I was very pleasantly surprised. It was unbelievable how much high school boys could grow and mature between their junior and senior years and I think our rival schools were also surprised because we began to win football games by rather large scores. We managed to win the [Wabaunsee] County League Championship and all nine of our regularly scheduled games without being scored on. It was fantastic (chuckles)! By this time the school and townspeople were really football mad.

We decided to challenge Council Grove, a somewhat larger school nearby, also the champion of their league, to a post-season game at the opening of a new highway. That was our downfall because the other team had twin brothers as halfbacks who were too fast for any of our players to catch. In any case, we lost the post-season game 16-7, and we therefore missed an undefeated season.

The basketball season was nearly as successful, with Eskridge High winning the county league championship and two post-season invitational tournaments. In the spring they made the sweep complete by also taking the league championship in track. It would have been difficult to top the 1924-25 year's record since most of our leading athletes were graduating from high school that year. I also made the decision that coaching in high school was not to be my permanent occupation. So I then returned to Iowa State University for graduate studies in plant pathology.

When I left, I was presented a beautifully engraved gold hunting case watch inscribed: "Presented to Coach Leach by the athletes of Eskridge High School, 1925."

For my eightieth birthday, Faris Edgar, who was the leading scorer on the basketball team, and his wife drove up from San Diego to Davis and spent the day with Jane and me. Naturally, we relived the excitement of our two years at Eskridge High School.

After graduation from Eskridge, Faris Edgar, whose father was a minister, moved to southern California, and Faris attended Santa Ana Community College where he was high scorer on their basketball team.

Haven't you also recently been in contact with several others who were in that high school class of yours long ago?

Yes, just in the last few years I've had several contacts with former students. This had been over 55 years since I was teaching and coaching in Eskridge High School near Topeka, Kansas, and I had never been back and had very few contacts. But the young man I've mentioned learned that I was here at Davis, so he called. We talked on the phone and later he and his wife visited us, and since then, we have seen them either here or in San Diego on several occasions. Out of that grew some contact with two of the young women who were students in Eskridge High at the same time, and they have come to visit us twice. Then I received a phone call from one of the young men who played halfback on my football team, and he indicated that he had been thinking about the times we had together and decided to call from Kansas.

He must have thought highly of you to do that.

Well, I think we had very good relations. It was through him that I was able to secure a copy of the annual which described all of the activities of the school during that year. In fact, this annual had been edited by one of the young ladies from San Diego who visited here. Then, very recently, I received a letter from one of the football players who is now living near Topeka, and he indicated that he had one son and he had named him Lyle because he recalled what good times we'd had together at the time I was coaching at Eskridge.

Those are nice contacts to have after so many years. Even your successes as an athletic coach apparently didn't keep you from returning to the classroom yourself. When did you begin graduate work?

· III ·

IOWA STATE: GRADUATE STUDIES IN PLANT PATHOLOGY

So, in the fall of 1925, after spending another summer eradicating barberries, I enrolled in graduate school at Iowa State. After two and a half years of being out of school, I found it somewhat difficult to settle down to the routine of a graduate student. I was assigned to Dr. S. [Samuel] M. Dietz, as my major professor. He was a plant pathologist with the USDA, in charge of the project on crown rust of oats and I served as his assistant for three years. I learned that buckthorn, or *Rhamnus cathartica,* serves as an alternate host to crown rust in the same way barberry does for black stem rust. There was no organized program for eradicating buckthorn, but one of our jobs was to develop the simplest methods of killing or eliminating the bushes where growers voluntarily wished to do so. We poisoned the bushes with salt, kerosene and other relatively nontoxic chemicals and tried mechanical removal. The results were published in a USDA circular in 1930.

FIRST PUBLICATION: BUCKTHORN ERADICATION

This was no masterpiece, but it was my first publication.

What was the title?

"Methods of Eradicating Buckthorn."

In the greenhouse, our research consisted of inoculating oat seedlings with collections of crown rust from different areas and we noted that a given set of oat varieties did not respond the same to collections of rust from divergent geographical areas. This led to a study of specialization in *Puccinia coronata,* the crown rust of oats.

We also collected the black spore stage of the rust on oat straw that had been stored outdoors overwinter and in the spring we inoculated leaves of buckthorn seedlings in moisture chambers, in a warm, humid, greenhouse atmosphere. After the cluster cup stage developed on the buckthorn leaves, we used the spores to inoculate a collection of oat varieties. The development of the rust on the differential hosts or varieties was not in all cases the same as the results from inoculation of the same differential hosts with the red spores stage from the same source as the black spores. It appeared, therefore, that passing the rust through the Rhamnus host in some way changed the pathogenic capabilities of the rust. Within two years this was explained by Dr. [John H.] Craigie of the University of Manitoba in the case of black stem rust on wheat and led to the understanding of heterothallism among the rust fungi.

That was a significant development?

Yes. For this discovery, he received the Eriksson prize from the American Association for the Advancement of Science. It is considered one of the outstanding research accomplishments in plant pathology. It was interesting to see how close we were to the information, even though we did not develop the explanation.

In any case, during the spring of 1926, I travelled extensively by car with Dr. Dietz, observing the southern limits of crown rust infection on Rhamnus species. We also collected data on the first appearance of crown rust on oats in the midwestern states and on the relation of Rhamnus infection to the local development of crown rust on oats in southcentral Wisconsin.

MASTER'S DEGREE, IOWA STATE COLLEGE, 1926

I completed my thesis for the Master of Science degree in August 1926, and was encouraged to continue graduate work toward a Ph.D. degree.

What was the subject of your master's thesis?

"Specialization in *Puccinia coronata,* the Crown Rust of Oats."

What living arrangements did you have at Ames?

I shared a room with another graduate student in the home of Dr. S. M. Dietz, my major professor, but I ate most of my dinners at the Kappa Sigma fraternity house.

Did your fraternity brothers help you get acquainted socially?

Occasionally they would arrange a date for me at a special party but most of my evenings and weekends were spent in the plant pathology department. However, during the summer of 1926, one of my close friends suggested that he knew a young lady with whom he felt I should get acquainted. He called her up and asked if she had a date for a certain Saturday evening. She accepted and then he explained to her that he was calling to make the date for a friend. So, I think she reluctantly accepted it and this was my first introduction to Jane Rhoads, who eventually became my wife.

Would you tell about your courtship?

Well, it started during that summer and we went to several dances, but I found that there was considerable competition. She had attended Ames for two years previously and then had stopped out for one year to teach in high school and was returning at this time for her last year at Ames. Nevertheless, I was able to arrange a date occasionally, and we became quite good friends. So during the following year, we had dates on a number of occasions and . . .

How many is that?

(Laughs) How many? I really didn't keep a calendar on that but . . . .

Twice a week, three times a week?

Well, usually I would see her two or three times a week and then one or two dates on the weekends.

What did you do on your dates?

Well, mostly we attended dances, and occasionally going out to dinner, and during the summer weather, it could be picnics.

Did you have a car?

Yes, this was one of my advantages, having worked for two or three years and being on an assistantship, I could afford a car where most college students at that time were on foot.

What kind of a car?

Ford, Model T. First I had an open touring car and later on, what we called a roadster.

With a rumble seat?

Mine did not have a rumble seat. I had a friend who had one of the first Model A's. It had a rumble seat and so I spent quite a little time there on double dates.

When did you pop the question?

Well this came, I think, early in the spring of '27. As part of my job I was sent on an extended trip to the southern states which included a trip to Oklahoma, Texas, Tennessee, Georgia and Alabama making surveys of rust, particularly on oats, and doing some research work at a station at Experiment, Georgia, just south of Atlanta. Since I was going to be away for about five weeks, we discussed this and she accepted my fraternity pin. This was the beginning of our engagement. This way I felt it would be safer during this extended trip. During the following year Jane accepted a job teaching at a small high school about fifty miles west of Ames and I continued taking course work and doing research with rust on oats during that year.

It was during this time that I first met Dr. J. [James] B. Kendrick, who was the first plant pathologist regularly assigned at the University of California at Davis. He was then on the staff of Purdue University but had accepted a job in California and he stopped at Ames on his way. I talked with him quite a little about his plans when he went to California.

In early spring of 1928, Dr. Kendrick contacted me and asked if I would be interested in being considered for a job that was opening up at the University of California. The job was to work on diseases of strawberry, particularly a virus disease known as xanthosis, meaning yellows. At first the UC people suggested that since I had not completed requirements for the Ph.D. degree, that, if I accepted the job in California, I could then do my research and complete my graduate requirements at Berkeley. The chairman of the department at Iowa State, however, suggested that it probably would be better if I would complete my qualifying examinations and my language requirements at Iowa State and then, if I accepted the job in California I could do the research for the Ph.D. thesis as part of my work there, submit the thesis back to Iowa State and thus complete all the requirements for the Ph.D. degree. This is the plan that was eventually accepted and,looking back,I can see that it was a great advantage to me to handle it in this way rather than to start all over at the University of California, where the requirements were quite different and, I would say, somewhat more difficult to accomplish.

Who was the chairman of the plant pathology department at Ames at that time?

The chairman was Professor Irving E. Melhus. He had earned his Ph.D. degree at the University of Wisconsin and was building a strong department at Iowa State in both graduate instruction and in research.

And who was the chairman of plant pathology at the University of California then?

Professor Ralph E. Smith was chairman. He came from the University of Massachusetts where he had earned his Master of Science degree.

Did he not have a Ph.D. degree?

No. He had not completed his doctoral degree but he had considerable research experience. When I reported for duty in his office in Hilgard Hall on the Berkeley campus on April 1, 1928, Professor Smith told me that by coincidence, twenty-five years ago that morning he had first reported for duty in Berkeley.

Was he a good chairman?

I would rate him an excellent chairman. Soon after he arrived at Berkeley (in 1903) he established the first separate department of plant pathology in the United States. All others at that time were combined with botany departments.

How about his research program in California?

His first project was the control of rust on asparagus, but he soon branched out into applied research on bacterial blight of pears (pear blight, or fire blight) and then on bacterial blight of walnuts, brown rot of lemons and several other diseases. His early successes attracted support from growers and administrators and he was able to add several staff members to his department.

Perhaps later on I can summarize in greater detail his accomplishments during his first years in California.

Was he chairman at Berkeley?

Yes, but he was also in charge of plant pathology at Davis and at the Deciduous Fruit Station near San Jose.

· IV ·

MARRIAGE, APPOINTMENT AT UNIVERSITY OF CALIFORNIA

Where were you first stationed once you decided to move to California?

The first few months I spent at the University of California's Deciduous Fruit Station at San Jose and I visited strawberry plantings where there were reported to be disease problems. I also inherited a collection of varieties and some hybrid seedlings that had been produced by my predecessor.

Could you describe the physical facilities of the experiment station in the San Jose area when you first came.

Once I reported at Berkeley, Professor Smith took me down to San Jose and indicated that it would probably be best to locate at the deciduous fruit station because it would be centrally located to most of the strawberry-producing areas in northern California. The station, at that time, was in a small cottage on Settle Avenue in Willow Glen, which is now in the south central part of San Jose. This small station had only four staff members working there—two entomologists and two plant pathologists. The station was a successor to a former station that was located at Moutain View, but because several of the staff members from the Mountain View station had been transferred to Davis, that station was closed and the activities were moved to this cottage in Willow Glen.

What staff members had been transferred to Davis?

This would include Professor [Walter L.] Howard, who, at the time I came to Davis, was the director of the experiment station and in charge of all the activities on the Davis campus. Also, Dr. [Frank] Veihmeyer, Dr. [Arthur] Hendrickson, the first in water science and the second in horticulture, had both moved to Davis. So those earlier activities had been terminated in the Santa Clara area.

How long had that station been located at Mountain View?

I don't know how long it had been there, but I know it was there for several years, and it specialized primarily on problems associated with apricot and prune culture. At that time, I think there were something like 20,000 acres of apricots in the Santa Clara valley, and an even larger area of prune production. But of course now, almost all of that area has been taken over with high technology industries and housing. The only trees that survive are trees still maintained in a few yards and the commercial production of prunes and apricots in the vicinity of San Jose is extremely limited.

Yes, that area has changed a great deal in the last few years.

It's now called Silicon Valley.

How long did your work on strawberry diseases continue?

I was at San Jose for about four months and then was transferred to Berkeley, where I worked from August 1928 to February 1929 when we moved to Davis.

You say "we." What happened to a lady named Jane Rhoads during this period?

That's very important that we pick that up. Before leaving Ames, we had discussed this problem. Since I would have just about exhausted my reserve of funds by the time I paid my expenses to California, it didn't seem very practical to turn around in July and come back to the Middle West for a wedding. So Jane agreed to come to California where we would be married around the last of July.

WEDDING IN BERKELEY, 1928

I know there was considerable reluctance on the part of her family and other relatives but it was worked out in this way. So during the last week of July, Jane came by train to Berkeley where I met her. At that time you had to file for a marriage certificate and then wait three days, I guess to see if you'd change your mind, and then you could get the final permit to be married. We were married at Westminster House in Berkeley, a Presbyterian student center on the corner of Bancroft and College Streets. A retired minister performed the ceremony and the only people who attended with us were two very close friends of Jane's who lived at Menlo Park and my aunt from Los Angeles who came up for the occasion.

What were their names?

George and Martha Nesbitt were the couple from Menlo Park. And my aunt was Mrs. [Jennie] Geisert, my mother's sister.

What was the date of marriage?

July 30, 1928.

Tell me, had you driven the old Model T out to California?

No, I left it there to be sold and I came on the Santa Fe Railroad by the southern route, directly to Berkeley.

After your wedding in Berkeley, were you able to take a honeymoon trip?

Yes. I had only been here about five months, but I was able to arrange with Professor Smith that I could take about two weeks off at the end of July. At first, we thought we would be living in San Jose after we were married, but it was just at that time that Professor Smith made the decision that it would be better for me to transfer to Berkeley where there would be better laboratory facilities for some of the work I was planning to do. So I had acquired a used car and following the wedding drove this car from Berkeley to San Jose, then over the Santa Cruz Mountains (on the old road), which for two people that were used to the flat lands seemed like quite a rigorous trip. From Santa Cruz, we drove north to Boulder Creek, and stayed at the Dickenson Lodge, right on the San Lorenzo River. We had a small cottage there and had our meals in the main building of the lodge. One night I remember that we went to the Brookdale Lodge, quite a famous location, where the restaurant was built over a small stream that ran right through the dining room. This stream was well stocked with fish so that you could sit there at your dinner table and watch the fish swimming up and down. The second week of our honey-

Jane and Lysle Leach, honeymoon in Carmel Highlands, California, August 1928.

moon we moved from Boulder Creek down to Carmel, and we rented a small cottage on the south side of Ocean Avenue, between the business district and the beach.

Did you see much of that area while you were there?

Yes, we drove extensively around the Carmel area. At that time Carmel was a very attractive village. We spent a good part of our time on the beach which has very white sand but the water was very cold and a bit rough.

Did you swim in it?

Yes, we were in the water quite a little. Until this honeymoon trip, Jane had never been to the ocean, and I had been there only on one or two trips just a few months before.

Did you get out on the water at all?

Well, the only time we were away from shore was while we were at Santa Cruz. We went on a deep sea fishing trip. Jane was quite a fisherman, and so we decided to use one day for this deep sea fishing trip. Everything worked quite nicely until they reached the fishing grounds and anchored. Then the boat began to bob up in regular rhythm, and one by one the passengers began to turn green around the gills and to show various stages of *mal de mere*. Jane soon lost interest in fishing. There was no space to go inside so she laid on the deck, or a little cabin roof, and ended up with a fairly good sunburn on one side of her face which had to be equalized later. It was not as successful a fishing trip as we had hoped.

And where did you live after you returned to Berkeley?

We first rented an apartment on Hilgard Street about four blocks north of campus.

RESEARCH STUDIES: STRAWBERRIES TO BEETS

This was quite convenient to the greenhouses where most of my work was being done and to my laboratory, shared with two or three other graduate students in Hilgard Hall.

What was your first appointment in the College of Agriculture and your beginning salary?

Well, my first salary with the University of California was $2,200 per year, which incidentally was a little less per month than I received teaching and coaching in high school. But I had hopes that the job would have more prospects for the future. My official title was Associate in Plant Pathology. This was a temporary title in the experiment station for persons who had not completed their Ph.D. degree.

Did you have any teaching responsibilities at Berkeley?

No.

You had the opportunity to continue your graduate work.

Yes. The research I was doing on strawberries was quite interesting but was quite foreign to anything that I'd done before and I found that my preparation was not adequate, particularly in laboratory phases. At Ames I had worked with rusts which are obligate parasites—they cannot be grown in culture, only on living hosts.

What does that mean?

They are obliged to live on a specific living host. They cannot be grown on artificial media. And so this meant that I had very limited experience in laboratory methods for isolating and testing fungus pathogens. I did recover some fungi from strawberries, particularly in the Watsonville area, that appeared to be causing a wilting disease of certain varieties. I purified the cultures and inoculated strawberry plants to determine whether this fungus was the specific cause of this wilt disease.

After we lived in Berkeley for approximately six months, Dr. Kendrick, who was located on the Davis campus, suggested to Professor Smith that it might be desirable to transfer me to Davis where there were several disease problems that might be more suitable for thesis research than the work I was doing on strawberries.

I learned from him about a very serious problem on garden beets that were being grown for seed, caused by a downy mildew fungus which would attack the leaves and then, by growing on the young heart leaves of the plant, it would spread into the crown and become systemic. When the garden beets were transplanted into the fields in early spring, for the production of seed, the fungus would essentially destroy the growing point and would greatly reduce the seed production on these plants. Dr. Kendrick suggested that this was a problem that was quite serious to the seed industry and would be a suitable subject for a dissertation for a Ph.D. degree.

The idea of transferring to the Davis campus and taking up a new area of research for your thesis appealed to you?

Very much so. I was somewhat lost on the Berkeley campus and I thought I was better prepared to fit into the more rural atmosphere of the Davis campus. So all of the strawberry work was turned over to Dr. Harold Thomas who had recently received his Ph.D. degree at Berkeley, and thereafter, all of my work at Davis related to Downy Mildew or to other diseases of vegetables and field crops.

I completed the research work at Davis and prepared a thesis in the spring of 1930. I then returned to Iowa State College to submit my thesis and to stand the final examination in June of 1930.

You'll be discussing your research projects in detail with Jack Hills. For now, let's talk about your move from Berkeley. When did you arrive in Davis, which is still your home?

On February 1, 1929.

RECOLLECTIONS OF A NEW HOME IN DAVIS, 1929

Can you recall where you lived, what Davis was like, etc.?

Well, Davis was a very small town of about 1,250 people. The campus at Davis, a branch of the College of Agriculture at Berkeley, had been started about 1906, primarily for work in dairy husbandry and dairy industry, and later developed in other fields of agriculture. In 1929, most of the students were in the two-year program and received a certificate, not a university degree. When I was considering coming to California, I talked with an agronomist with the USDA who had been in California on a few occasions and he said, "Well, if you go to California and are located at Berkeley or San Jose, it probably would be all right, but God help you if you're ever transferred to Davis." (Laughter.) So that was my first introduction to Davis.

We arrived, as I said, on the first of February and it was one of these cold, damp periods with ground fog, or tule fog present practically all day.

Like now.

Yes, just like today. And this persisted for the best part of two weeks, so we had a very poor first impression of the climate here.

Where did you live?

We first lived at the Terminal Hotel, which still exists on the corner of Second and G Street, and Jane spent a good part of her time searching for a permanent residence. We found only two vacancies in Davis. One was a small apartment attached to a house, and this was not a very inviting place for a young couple to set up housekeeping. The other, which we rented, was a very old house with poor heating facilities but which was located closer to campus. This house still exists just a block from the campus. By now it must be over seventy-five years old.

What's the address?

It was between Second and Third Streets. I think the number was 212 University Avenue. We took this house because it was about the only thing available, but Jane spent a good part of her time looking for something more suitable. At Berkeley, we had paid fifty dollars a month for a very modern, new apartment. This first house we rented at Davis was a very old building, not modernized in any way except for the bath facilities which were inside, at least. We paid thirty dollars a month there. Heating facilities were very unsatisfactory in this old home, so after about three months Jane was able to locate a small four-room cottage on Second Street, at that time directly adjacent to the campus. Now the area is included in the campus and I can locate the site of our small cottage by a very large elm tree that grew in the back yard. There, we also paid about thirty dollars and we lived in that cottage for about two years before we were able to move up to a little better house.

Was it a problem living that close to the student dormitories?

Not really. There were only about 400 students enrolled on the campus, with only a part of these living on campus. I don't remember that we had any problems as a result of living so close. We were just about 200 yards directly east of North Hall, which is still there, but no longer used as a student residence.

Didn't you once have trouble with the University's horses running through your yard?

What happened was that during sunny weather it was our custom to locate a small playpen in the front yard, and this was occupied by Joann, our first child, who was then about one year old. One of the farm wagons with two large Percheron horses was being used on campus to pick up trimmings and trash, and for some reason, these horses were spooked and started to run away. They came directly down Second Street in front of our house and one of them turned in through the opening in the front hedge and the other stayed on the outside. So they came directly across the corner of our front yard. Fortunately, Jane had moved Joann indoors and brought in the playpen just about a half hour before this happened. Otherwise we would have had a real catastrophe with a runaway team of horses.

Did you move other times before you built your own home?

Housing was so tight in Davis that whenever someone would leave, or build a home, or move to another location, then almost everyone would move up a little. So whenever we found a home that was a little larger or a little more attractive, we moved—we lived in seven different houses between 1929 and 1938.

What were some of the other locations? Are any of those houses still there?

Yes, one of them was a house on the north side of Russell Boulevard [225], just across from the campus. That is now occupied by a sorority. Two of the cottages that we lived in were on the east side of A Street, directly across the street from the northeast part of the campus. One of these houses is still standing [324 A Street] ; the other one was torn down and a bookstore is occupying that site [230 A Street]. The last one we lived in before we built our own home was about eight blocks northeast of the campus, at 507 E Street.

At one point didn't you live out in the country?

While we were living in one of the cottages on the east side of A Street, the owner decided that she wanted to occupy this house herself, and therefore she gave us notice to give up our lease in July of that year. There were no other houses available at this particular time, so we located a cottage on the south side of Putah Creek, about seven miles west of Davis, near Stevenson Bridge.

Was there nothing available in the whole town of Davis, or did you want to live in the country?

There was nothing available.

So how long did you live out there?

This was a matter of about five months, from May until late September. Both of our children were in school at that time, and had to be picked up at different hours. Once school started in the Fall we were all on different schedules. There were no school buses at that time. We had only one car and it was necessary to drive to and from town several times each day.

So your wife became a taxic cab driver?

That's right, and for this reason we saw that it was very important that we find a place in town again. We were able to rent that house on E Street, but we saw that the problem of housing in Davis was going to get nothing but worse. So we decided we had better get a lot and make plans for building our own house, because by this time it began to look as if we might be staying in Davis for some time.

You were able to purchase a lot, then. Who designed your house?

Yes, we bought a lot on the south side of Rice Lane, from the Plant estate. Professor Ed [Edward B.] Roessler, who is professer of mathematics, had been trained in architecture and offered to work with us on the designing of the house and drawing of the plans that could be submitted for bids. So he was the architect on the house and we thought he did a very fine job of planning the space that we really wanted. Ed McGuire, a local builder, was the general contractor.

You built a house of used brick; that was unusual at that time, wasn't it?

Yes, it was, but we found an opportunity to buy a quantity of used brick that was too good to pass up. The University, in about 1937, had determined that the Classroom Building on campus had to be condemned because it settled on one corner and was no longer safe to use for large group meetings or classes. This building contained about 300,000 used brick. We were able to purchase 15,000 of these, cleaned and laid down on the lot, for nine dollars a thousand.

Mr. McGuire subcontracted with the Redmond Brothers who were very fine bricklayers from Sacramento. And so they used the 15,000 used brick for the facing of the entire house and for the building of a very large double chimney for two fireplaces.

And this is the home that you still live in today?

Yes, we've been living here since December 1938 and find it very well built and very comfortable.

Well, going back to 1929, I wonder if you could recall what downtown Davis looked like, the business district of the town.

The town had a population of only about 1250. The main street [G Street] on the west side was a series of mainly one-story businesses. This included a hardware store, a bank and a post office, and a meat market and some general supply stores. The east side of the street consisted of the Terminal Hotel, which is still standing, and a garage with open space on both sides of it. Someone referred to it as the backdrop for a western movie, which I think was a fairly good description of the east side of G Street at that time.

There were no large stores at all?

The largest was Roger's, which was a general supply store. They handled groceries, clothing, some furnishings and general supplies.

Could you kind of recreate living here in 1929?

That is a large order. I'm quite sure at that time Cal Covell was the mayor of the city, and he is remembered by Covell Boulevard.

Which is mispronounced CO vell, as in "Oval."

Yes. The newspaper was the *Enterprise,* and Judge [Wm. H.] Scott was the editor and when the weekly came out it was always the source of amusement to read the articles which were reproduced in this paper.

What were they?

Judge Scott wrote in a very informal manner and I remember a wedding that he was reporting, the bride was a daughter of a well-known Davis farming family, and Scott in reporting the details of the wedding digressed to tell how this ranch about two or three miles out of town sheltered many wild animals, including grizzly bears. He went into great detail about the bear hunts that had been held on her family's ranch. We thought it was quite amusing to include all of this in the wedding reports.

Was it true?

Yes, it was.

Where was this ranch property located?

The land was owned by the Montgomery family and included the present El Macero Country Club, and several other ranches in that vicinity southeast of Davis.

What about social activities? Was it easy to get acquainted in Davis and what sort of things did faculty people and their wives do for entertainment?

The faculty was small in number so it was quite easy to get acquainted. The most central activity seemed to be around the Faculty Club, which had rooms for pool and billiards and for card playing. The Faculty Club would put on various kinds of activities, like bridge tournaments, and at Christmas time, their big event of the year was the so-called Goose Stew. Almost everyone connected with the University at that time planned to attend the Goose Stew, or Christmas party.

That's still an important event.

Yes, it is. Then in the spring they had an annual Steak Bake, which sometimes was held on campus and sometimes at park areas along Putah Creek, about twenty miles west of Davis. I don't remember that it was boring at that time because there were lots of things to go to–a lot of private parties, bridge games, small dances. We kept quite busy.

Did you continue your interest in athletics?

Shortly after my arrival in Davis I found that there were two prominent members of the staff at Davis who were actively engaged in officiating football and basketball among the high schools and junior colleges of this area and they encouraged me to join them in this activity. They were, first, Deming Maclise, the business manager of the Davis campus, who later became the comptroller at UCLA, and Professor Stanley Freeborn, who at that time was vice-chairman of the Department of Entomology and who later became assistant dean of the College of Agriculture at Berkeley, and in 1952 was appointed the first provost of the Davis campus. Later his title was changed to the first chancellor on this campus.

I joined the officials association in Sacramento, and after some study, training and experience, I received assignments to officiate high school games. Very often in the early days I was assigned with Deming Maclise and Stan Freeborn. I officiated games in the Sacramento Valley ranging from Merced on the south to Redding on the north and also in a number of junior college, now called community college, games. I was also assigned to officiate at college games such as the College of the Pacific, now University of the Pacific, University of Nevada, and Chico State College. I officiated football from 1929 to 1942, and refereed a large number of games played in Hughes Stadium in Sacramento and in the larger towns of this area. Basketball I had found a little too demanding because of the closeness of the crowd and the necessity to make instant judgments. Because I did not find it really enjoyable, I stopped officiating basketball after four to five years.

Regarding your family, when was your first child born?

This required only one year. Joann (Eleanor) was born on July 27, 1929, three days less than one year after our marriage.

And subsequent children?

Doug (Douglas Rhoads) was born on January 29, 1932 and Janet (Katherine) much later, in 1945 (on July 2).

We'll speak more about your family life in subsequent interviews, but this will give us a reference to their names and ages.

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FIRST IMPRESSIONS OF THE UNIVERSITY FARM

Would you describe the campus when you came.

They were just undertaking the paving of the roads. Up to this time the roads on campus had not been paved. The quadrangle was in alfalfa and most of the buildings were old redwood buildings with shingled siding. These consisted primarily of North, West and South Halls which housed the students; the dining-infirmary building, later relocated and called East Hall; and a brick classroom building located where the first section of the library now stands, just south of the quadrangle. The animal husbandry building [Animal Science] which had just been completed and the horticultural building occupied the area west of the quad. A number of smaller wooden buildings were located west, south and southeast of the central area. Plant pathology occupied a small cottage located directly west of the Classroom Building, near the southwest corner of the central "Quad."

CAMPUS ADMINISTRATION

How many faculty members were at Davis in 1929?

There were perhaps a hundred teachers and experiment station workers and about 400 students, as I've mentioned.

Were there any women on the faculty then?

Yes, there were a few, primarily teachers in English, mathematics or science departments.

Who was the senior officer of the campus?

At the time I came here,Professor [Walter L.] Howard was the director of the Davis Branch of the College of Agriculture and of the Agricultural Experiment Station. Earlier, Claude B. Hutchison had been the director [1922–1924]. Then he left for a period of two or three years with the Rockefeller Foundation in Europe. When he returned to the University of California, he became dean of the College of Agriculture at Berkeley [in 1928], and later [in 1951] the first vice-president of agricultural sciences.

Who was the business manager at Davis?

When I arrived, Deming Maclise was the business manager. After a few years he was offered a similar position at UCLA where he was designated as comptroller. Then Ira Smith, who was the assistant to Deming Maclise, took over as business manager on the Davis campus.

Would you say a few words about Ira Smith.

Well, Ira Smith was what I would term a strong administrator. He had very strong control over the budgeting and the fiscal operation of the departments. He had under his control the engineering or farm structures, and he had very strong control over the students because he was

Left: Referee Leach in the '30s.

Above: UC plant pathologist Leach prepared for field work in the early '30s.

administering all of the student housing and dining facilities. He worked very closely with the strong department chairmen, and I think it was a very effective arrangement. Some people felt that he exercised too much control and there was some friction between Ira Smith and some of the departmental chairmen. As time went on, the authority on the campus was distributed so that when Knowles Ryerson was appointed as assistant dean of the College of Agriculture on the Davis campus [in 1937], some of the authority previously exercised by Ira Smith was transferred to Dean Ryerson's office. But Ira Smith had very close association with University President [Robert Gordon] Sproul and with Dean Hutchison and always exercised a great deal of authority.

There were several strong departments on the campus, and most of them were headed by chairmen who had a great deal of authority in their area and operated in their own field quite independently. There were very few restrictions from the University and so the departments were responsible for the employment, for the budgeting, and for the full operation with the minimum of central control.

Can you name the departments?

Agronomy, animal husbandry, agricultural engineering, dairy industry, pomology, poultry science, truck crops (later vegetable crops), viticulture. In addition to the production departments, there were a few staff members in entomology, plant pathology, veterinary science and several instructional departments such as botany, chemistry, English, mathematics, physics, zoology, etc.

Who were some of the campus leaders?

Dr. Walter L. Howard was the director but most of the campuswide authority rested in the office of the dean of [the College of] Agriculture at Berkeley. Some of the strong chairmen were Ben Madson, agronomy; George Hart, animal husbandry; H. B. Walker, agricultural engineering; Warren Tufts, pomology; Henry Jones, truck crops; A. J. Winkler, viticulture; Wilfred Robbins, botany, and Tracy Storer, zoology.

DEPARTMENT OF PLANT PATHOLOGY: FACULTY OF TWO

What was the status of the department of plant pathology when you arrived at Davis in 1929?

Professor Smith had established the department at Davis in July 1927 when Dr. [James B.] Kendrick was hired. Dr. Kendrick was vice-chairman in charge at Davis. Staff members from Berkeley were occasionally assigned to work at Davis for temporary periods. I was the second plant pathologist who was regularly assigned on the Davis campus.

What was Professor Kendrick's official title and the subject of his initial research?

He was associate plant pathologist and vice-chairman under Ralph E. Smith, later under Dr. Gardner when he became chairman of the joint department in 1936. Dr. Kendrick's thesis research was on a bacterial leaf spot disease of sorghum. His later research at Davis was on various diseases of vegetables and field crops. He graduated from Clemson [College in South Carolina], and he came to Iowa State College and finished a master's degree and then his Ph.D. degree. He had accepted a position at Purdue University with Dr. Gardner, and he continued at Purdue until 1927 when he came to California.

Wasn't he the father of Jim Kendrick, who is now the University's vice-president of agricultural sciences?

Yes, he was the father of James B. Kendrick, Jr., who was in plant pathology at Riverside for a number of years and is now the vice-president of agricultural sciences, located at Berkeley. A younger son, Edgar L. Kendrick, is a plant pathologist with the U. S. Department of Agriculture. He is now regional administrator for the USDA, located in New Orleans.

After Dr. Kendrick and you, who were the next faculty appointments in plant pathology?

The third member of the Davis staff was Dr. Edward E. Wilson, who received his Ph.D. from the University of Wisconsin. He took charge of diseases on deciduous fruits and nut crops when he arrived July 1, 1929.

INITIAL RESEARCH ON SUGAR BEET DISEASES IN CALIFORNIA

Were you the first plant pathologist to work with sugar beet diseases?

Professor R. E. Smith and R. L. Adams studied the curly top disease on sugar beets and Dr. [H. P.] Severin, an entomolgist, studied the leaf hopper that transmitted the curly top virus.

Who besides Dr. Kendrick influenced your research on beet diseases as a subject for your Ph.D. thesis?

I had considerable help from Dr. Harry Borthwick in botany and later from Dr. Katherine Esau on anatomy of beet plants.

Wilfred W. Robbins was the chairman of botany and was very well known throughout California because he was an excellent public speaker and very amusing. He had come from Colorado and in 1929 all of the research on sugar beets at Davis was under Dr. Robbins' direction. He had worked with the Great Western Sugar Company back there and had a good deal of experience with sugar beets in Colorado.

How did that relate to your work?

When I started research on downy mildew of garden beets, which I used for my thesis, I found that the same downy mildew occurred quite commonly on sugar beets along the coast, particularly at Salinas and Santa Maria. It was perhaps the second most important disease occurring on sugar beets in that area, so it was quite natural to move from work on the garden beet seed crop over to the commercial crop of sugar beets.

In 1930 I had my first field plots at the Spreckels Sugar Factory just out of Salinas. Dr. Robbins invited me to accompany him to the second meeting of the California Sugar Beet Growers Association, which was held in the early spring of 1931. And incidentally, the fiftieth anniversary of that association is being held in February 1981 at San Diego.

Are you going?

Yes, I'm planning to attend. I can only think of about three or four people who were at the early meetings who will be present at San Diego. That would include George Wilson of Clarksburg and Gordon Lyons, who was the first manager of the Beet Growers Association.

So you and Dr. Robbins got along fine?

Yes, we did. He gave me very strong support because he was interested in the sugar beet industry, at a time when few others on the campus seemed to be. He seemed very glad to have me go with him on trips and to talk to growers groups. It was from this encouragement that I really got my start working with sugar beet diseases.

In completing the work for my thesis, I found that the overwintering stage of the fungus occurred in the leaf tissue and actually in the beet seed clusters, in the form of oospores. Oospores are formed as a stage of the downy mildew by the fusion of male and female sex structures. So the oospore is a sexual spore, has a very heavy wall, lives for long periods of time, and in most downy mildews, serves as a means of carrying the fungus over from one year to another. Therefore, I needed to study the detailed relations between the fungus and plant tissue, particularly the seed structures. My prior training was somewhat deficient in the areas of plant anatomy and physiology and so I appealed to staff members in botany to give me some help in this area.

Including Robbins?

Not so much Robbins, but Miss Katherine Esau who was very new to the campus. She was really outstanding in the study of plant anatomy. Later, she authored the most widely used plant anatomy book and, in my opinion, was one of the finest scientists on the Davis campus during those early years.

Also, Harry Borthwick, who was doing a good deal of the teaching in the botany department, was very helpful in clearing up some points concerning the structure of the sugar beet seed clusters and the relationship of the fungus. The sugar beet seed originally was a multiple cluster of fruits and when you planted a seed ball you could get anywhere from one to as much as five or six seedlings. It was in this structure that the Downy Mildew developed and produced oospores. This raised the question whether the infection originated from the oospores or from some other source. That is an area that I did considerable work on for my thesis.

PH.D. THESIS, FACULTY APPOINTMENT IN THE DEPRESSION

And what was the title of your dissertation?

The subject was "Biology of Downy Mildew on Beets," both garden beets and sugar beets.

And you returned to Ames to get your Ph.D. from Iowa State?

In May 1930 Jane and I along with our daughter Joann traveled by train to Ames, Iowa, where I presented my thesis. After much rewriting, my thesis was accepted and I passed the final oral examination. My Ph.D. degree was conferred in early June 1930.

We purchased a new Pontiac sedan and after a few days of visiting we left for California, with Jane's sister Betty joining our group. We returned by the southern route so we could visit my parents at Arkansas City, Kansas. Enroute we visited the Grand Canyon in Arizona and drove to Los Angeles where we were met by our mutual friend Bogue McSpadden, nephew of Will Rogers. We met Mrs. Rogers in Beverly Hills and she invited us to visit their new ranch home in Santa Monica, now the Will Rogers State Park.

From there we drove north to Goleta, just north of Santa Barbara, where Bogue was manager of the Spaulding ranch, then returned to Davis.

Lysle, while you were doing this first research project, what was your capacity and your status in Davis?

My title for appointment was associate in plant pathology in the University Experiment Station.

And why did you get that title?

This was an appointment that was given to people working at the experiment station who had not yet completed their Ph.D. degree. I came to Davis at a salary of $2,200 a year, and at the end of three months my salary was increased to $2,400 a year. Normally, after completion of the Ph.D., a person would be advanced to assistant plant pathologist, at a salary then of $3,000 a year. Unfortunately, the depression started by that time, and all advancements were slowed up. In 1929 I was appointed instructor and junior plant pathologist and my salary increased to $2,600 a year. Then in 1932 everyone received a salary reduction which was called the 2-10 cut; that is, salaries were reduced two percent on the first $2,000 and ten percent above $2,000. So, I received a small salary cut and an additional delay in receiving a permanent appointment.

You completed your Ph.D. in June 1930. When did you become a "regular" member of the faculty at UCD?

In 1934 I was appointed assistant professor and assistant plant pathologist. This was my first permanent appointment and my first academic title.

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DEVELOPING DEPARTMENT OF PLANT PATHOLOGY, UCD

HISTORICAL PERSPECTIVES; RALPH E. SMITH, PIONEER ADMINISTRATOR

You wrote the history of your department that was published in the University's Centennial Record *of 1968, and I believe you've done some additional research on its early development since then. What sources of information were most helpful?*

The two most important sources of information were, first, a manuscript by Ralph E. Smith titled "The Beginning of Modern Plant Pathology in California," and secondly a manuscript prepared by M. W. Gardner and J. B. Kendrick, Sr., titled "Ralph Eliot Smith, 1874–1953." This was written as a memorial.

Another useful source was a manuscript titled "Courses in the Department of Plant Pathology, Berkeley" prepared by M. W. Gardner in March 1972.

Professor Smith's memoir contains a lot of interesting information about legislative support for the University's agricultural research and about Professor E. J. Wickson's role in locating the University State Farm at Davisville in 1906. Did you ever discuss with him this early period?

As I've mentioned, this subject first came up the morning I reported for work at Berkeley in 1928, when Professor Ralph E. Smith told me that exactly twenty-five years ago that morning (April 1, 1903) he had first reported for work on the campus of the University of California. Smith came from Massachusetts to work on the control of asparagus rust. Asparagus was then primarily located in the Milpitas area north of San Jose and along the Sacramento River from Yuba City down to the delta region near Stockton. Rust was very severe and growers were looking for someone who could control this disease. Smith had worked with asparagus back in Massachusetts and so he undertook this research job. He found rather quickly that sulphur dust applied to the fern of the asparagus gave very satisfactory control.

The very next year an infestation of pear blight broke out in very severe form in the pear trees along the Sacramento River, and Mr. Smith contacted Dr. M. [Merton] B. Waite at the U. S. Department of Agriculture in Washington, D. C. Dr. Waite came to California and decided the best way to control this was to get a group of people who would visit the pear orchards and make arrangements to cut out all of the infected branches or twigs. So Dr. Waite sent five or six men from Washington to work on this project, and Mr. Smith hired five of his former students at Massachusetts. They also recruited a few people locally and ended up with about twelve young college graduates who were out working on the control of this pear blight, or fire blight as it is sometimes called.

From newspaper accounts of 1904 to 1907, these combined teams of USDA and University personnel worked extensively in Sacramento Valley orchards.

Yes, they stayed in the pear growing areas and worked directly with county horticultural commissioners and with farmers, training the farmers and their workers how to recognize the infected branches of the trees and how to cut out this as a means of removal of the inoculum that would otherwise lead to additional infections. All these prunings, then, were collected, dried and burned to destroy the inoculum. This was the best control that was known at that time and it did help to stop the epidemic.

Didn't a number of the group that Professor Smith and Dr. Waite trained during that blight work of 1904 and 1905 stay on to make major contributions to both the University and the state Department of Agriculture?

Yes, it's true, but very few of them stayed in plant pathology. One of them, R. [Richard] L. Adams, got his start with the pear blight control work and ended up as the head of the Department of Farm Management. Two others who first worked and taught in plant pathology later became the first geneticists in the University of California. These were Professor [Ernest B.] Babcock, Professor Smith's first student in plant pathology, and Professor [Ray E.] Clausen, and both of them had their start in plant pathology but branched out into the field of genetics. Stanley S. Rogers was another of that 1905 pear blight crew who joined the plant pathology staff. I'll speak more about him later, because in 1912 Professor Smith transferred him to Davis.

After coping with the pear blight infestation, Professor Smith got involved in walnut blight, a similar disease affecting walnut, and from that he was asked to work on a disease of lemons, a brown rot of the fruit when it was in storage. Here he found that the fungus causing this rotting was living in the wash water used to wash the lemons. So, while the lemons were cleaned, they were also being inoculated with the fungus. By putting a little copper sulphate into the wash water, he was able to stop the multiplication of the fungus and prevent the rotting of fruit. In just a very few years he was able to work on a number of disease problems and develop quite a reputation for himself for solving a number of important plant disease problems here in California.

The year 1905 seems to have been a landmark one. Agricultural interests also promoted the establishment of the University State Farm and some experiment stations in southern California then.

Yes, the University Farm had been proposed. They were studying where this should be located. Professor Smith's account notes that the survey was entrusted primarily to Professor [Edward J.] Wickson, a horticulturist on the Berkeley campus [who was Dean of the College of Agriculture, 1905–1912]. He studied a large number of sites and eventually it was decided that Davisville would be the choice location for this new agricultural school. And that was the beginning of the Davis campus.

The rest is history. Wasn't Professor Smith involved in the early beginnings of the Citrus Experiment Station in southern California, which the legislature also authorized in 1905?

Yes, about the time the Davis campus was selected as a site for the branch experiment station it was also decided to set up two laboratories in southern California. One was a laboratory [Southern California Pathological Laboratory] at Whittier and one was at Riverside, first at the Rubidoux Heights station and later moved a short distance away to the present site of the Riverside Citrus Experiment Station, which eventually was expanded to become the UC Riverside campus.

Professor Smith was put in charge of both of those laboratories [1906–1912] and he had a great deal to do with the first staff members who were located at Riverside and at the Whittier station. He was also in charge of the first experiment station in Imperial Valley.

Professor Smith seems to have been very successful in making plant pathology popular with the farmers of both northern and southern California. He was especially successful in getting research funds from what he termed "the beneficient legislature of 1905." During those early years before the Agricultural Extension Service was developed, he seems to have been a very effective ambassador for University research with both the agricultural community and the California legislature.

This is true.

Those manuscripts you've mentioned provide interesting reflections on California agriculture and University teaching and research at the turn of the century.

I should add that during the early 1900s all of the teaching and research in plant pathology in California was centralized at Berkeley, supplemented by a few field stations. Later, as I've noted, Professor Smith was instrumental in developing a strong plant pathology group on the experiment station at Riverside and on what was originally called the University Farm at Davis.

When Professor Smith started plant pathology at Berkeley in 1903, he was given the title of assistant professor of plant pathology and was the first person in the United States to have a professorship in plant pathology. Up to that time, all the departments had been combined with botany and most of the faculty members had titles as professor of botany, or botany and plant pathology. So he had the honor of starting the first separate department of plant pathology in any American university and also of having been the first to have had a professorial title in plant pathology. Smith's work was not very well known in the eastern United States because many of the specialty crops that he worked on here, like walnuts and apricots, were not grown in many other states. And he was so busy solving problems here in California that he did not participate actively in the pathology programs at the national meetings with the people in the eastern part of the country. Although he was one of the charter members of the American Phytopathological Society, he was not very well known in the middlewest and in eastern states. With so many disease problems to solve in California, his publications were primarily bulletins of local importance.

How long did Professor Smith serve as chairman?

He continued as chairman of the combined Berkeley-Davis department until 1936, when he relinquished his administrative title. But he continued in research until he became professor emeritus in 1944.

Wasn't he honored in April of 1953 at a dinner commemorating the fiftieth anniversary of the department of plant pathology?

Yes, that dinner was held in Berkeley. I was unable to attend because I was away at a dean of students meeting in Michigan, but I recall sending him a congratulatory telegram thanking him for his help and guidance. He died soon after that occasion on December 15, 1953.

SUCCEEDING DEPARTMENTAL CHAIRMEN, 1936–1980

In 1936 Professor Max W. Gardner, who had come to Berkeley in 1932 from Purdue University where he had been departmental chairman, succeeded Professor Smith. Dr. Kendrick continued as vice chairman at Davis. Since Gardner and Kendrick had been colleagues at Purdue and remained close friends, this was a very harmonious arrangement.

There was no friction between plant path faculty members on the two campuses in those days?

I don't remember any controversy between faculty members, probably because there were so few of us and so many problems to solve.

How did the 1952 Regents' decision to appoint a provost and a separate dean of the College of Agriculture on the Davis campus affect the administration of your department and the campus in general?

The administration of the department remained the same with Dr. Gardner at Berkeley the chairman and Dr. Kendrick as vice chairman at Davis serving under him.

With separate deans of agriculture at Berkeley and Davis, many more decisions concerning budgets, appointments and promotions were made on the local campuses.

During the 1930s and 1940s, undergraduate instruction developed on the Davis campus and at UCLA, closely followed by graduate courses. With the development of the campus at the University of California at Riverside, the plant pathology department also developed a strong graduate program there.

In about 1960, it was decided to terminate all research and teaching in agriculture on the UCLA campus and these activities and staff members were transferred primarily to Riverside, with a few going to Berkeley. Earlier it was often discussed whether it would be desirable, in view of the crowded conditions at Berkeley, to transfer all, or most of agriculture from the Berkeley campus to Davis. This was done with veterinary medicine and food technology, but active departments continued at Berkeley in agricultural economics, entomology, plant pathology and soil science.

In 1954 Professor Gardner retired and Professor Kendrick became chairman of the combined Berkeley-Davis department, with Professor [John W.] Oswald transferring to Berkeley and serving as vice-chairman. Not long after his appointment as chairman, Dr. Kendrick was told that most of the future expansion within the department would be on the Davis campus, with the Berkeley group remaining strong but at about the then present staffing numbers. Accordingly, Dr. Kendrick made several recruiting trips and arranged to add several faculty members at Davis. Some members of the department at Berkeley felt that they were not being treated fairly and made strong representations to the administration at Berkeley. This resulted in the appointment of a joint committee to study the question. The final report concluded that the programs at Berkeley and Davis were sufficiently different to complement each other, and recommended that the department be continued on both campuses.

When Dr. Kendrick retired in 1960, the group at Berkeley proposed to University Dean Dan [Daniel] Aldrich that because they had been unfairly treated in the past, the next chairman of the combined department should be from the Berkeley group. After some deliberation by the administration, I was appointed chairman over the combined department, with Dr. William Snyder vice-chairman at Berkeley. It was evident from the first that the group at Berkeley wished to make independent decisions and I never had the feeling that I was serving effectively as their chairman.

It was no surprise, therefore, when they petitioned to establish a separate Department of Plant Pathology on the Berkeley campus. With the approval of the group at Davis, I supported their petition. In 1963 two separate departments were established on the Berkeley and Davis campuses and it has remained this way to the present time.

You served as chairman of the joint departments at Berkeley and Davis from 1960 to 1963, and from 1963 until your retirement in 1968 as chairman of the department at Davis only.

Yes, that is correct.

Who has held the chairmanship since 1968?

Professor William [B.] Hewitt succeeded me as chairman in 1968, but the following year he was appointed associate director of the University Experiment Station and transferred to the Kearney Horticultural Station at Parlier, Fresno County. Professor Ray [Raymond G.] Grogan served as chairman from 1969 to 1973, Professor Tsune Kosuge served from 1973 to 1980, and Professor James E. DeVay still holds that position.

EXPANDING FACULTY, STAFF AND CAMPUS FACILITIES

What buildings were occupied by your department through the years?

The development of the department seems to have been very closely related to the housing. From 1927 to 1929 plant pathology occupied a small four-room cottage located directly west of the old Classroom Building (near the northwest corner of the present Shields Library.)

Might that have been one of the four original campus buildings? In 1907 they built a residence for the creamery manager that stood near the site you described.

I think that was the building. It was a four-room cottage with fireplaces in two or three of the rooms. Plant pathology moved from that building in April 1929 into the Old Animal Science building [constructed in 1910]–the wooden shingled building to the southwest of the library. There was a wooden water tank mounted perhaps twenty-five feet above the ground, located just behind that first cottage and just to the southwest of the Classroom Building. The tank house was taken down around 1929 or 1930; the cottage, I believe, was torn down.

John Jacobson's recollections note that it was sold.

Could be–I don't remember what happened to the cottage after April of 1929.

I should add that from articles compiled in recent years I have learned about several plant pathologists who were assigned to the Davis campus before Dr. Kendrick started the department in 1927. The first was Stanley S. Rogers, one of the young men that Professor Smith brought from Massachusetts to work on the pear blight problem in 1905. By coincidence, during the first few days I was in California staying in Berkeley at the Whitecotton Hotel–now called the Shattuck–I met a man in the lobby and we got to talking. He asked what I was doing here and I told him I had just come to take a job in plant pathology, and he said he had done the same thing in 1905. We [Professor Rogers and I] compared notes on this occasion, but it was years later when I learned that in 1912 Professor Smith had transferred Stanley Rogers from the Whittier station to Davis, had given him the title of instructor in plant pathology, and that he had taught courses on the Davis campus during two years from 1913 to 1915. In 1915 Professor Rogers transferred into the truck crops [vegetable crops] department and at one time held the title of professor of olericulture. Later, he left the University and accepted a position with the state Department of Agriculture in Sacramento.

During the years that followed, several staff members from Berkeley were transferred to Davis on temporary assignment. Dr. [P. D.] Caldis spent some time at Davis working on a disease of figs called endosepsis, or internal rot. However, he worked in the pomology department until he resigned in 1926 to accept employment with the California Packing Corporation, now the Del Monte Corporation. In 1926 Dr. [N. C.] Goldsworthy was transferred from Berkeley to Davis to work on bacterial gummosis of stone fruits. He remained at Davis until 1929 when he resigned and went with the USDA. Dr. [Hans Nicholas] Hansen came from Berkeley to Davis to continue the work that Dr. Caldis had started on fig endosepsis. Both Goldsworthy and Hansen were housed with Dr. Kendrick in that small cottage I've spoken about.

When I came to Davis in February of 1929, I was the first permanent staff member to work here with Dr. Kendrick. A few months later, on July 1, 1929, Dr. Edward E. Wilson came from the University of Wisconsin to take charge of research on fruit tree diseases. And when the animal science department moved into their new building at the southwest corner of the Quadrangle, their previous quarters were reassigned to plant pathology.

I recall that I was assigned to the office formerly occupied by Professor Elmer H. Hughes, the University's swine specialist, and for the next nineteen years it still said "SWINE" on the door of my office.

So plant pathology had a department of three faculty members when you first occupied the building that today houses the Department of Philosophy?

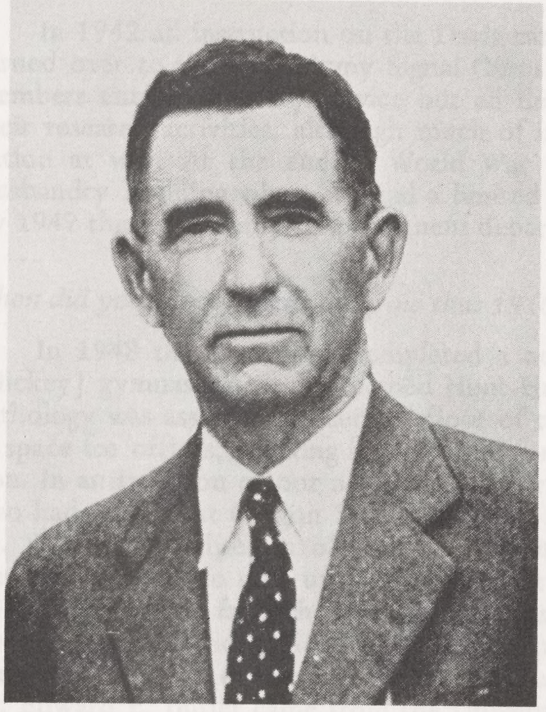
Yes. Then in 1934, there were two very bright young men in my Plant Pathology 120 class. William B. Hewitt was completing a Master's degree in pomology, and Byron R. Houston was completing his Bachelor of Science degree in plant science. Both were exceptionally good students, and became very interested in the field of plant pathology. So, the following year both transferred to graduate studies in plant pathology. Hewitt completed his Ph.D. degree in 1937 and was employed as a junior plant pathologist at Davis to study a very serious grape disease which was later known as Pierce's disease. Houston took his Ph.D. degree in 1939 and was employed in the Department of Plant Pathology to conduct research on diseases of field crops. At that time, all graduate students on the Davis campus were required to spend at least one year on the Berkeley campus to qualify for a Ph.D. degree.

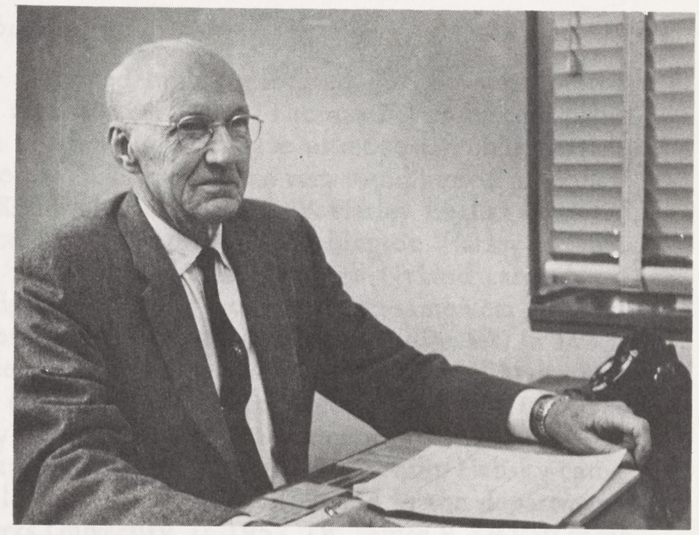
In 1938, John W. Oswald came to the Davis campus from DePauw University to enroll as a graduate student. He took most of his course work on the Davis campus and then transferred to Berkeley in 1940 where he received the University medal for the highest grades in biological sciences. He returned to Davis in 1941 and completed his thesis with the assistance of Dr. Snyder from the Berkeley campus, and immediately went into the Navy in 1942. He served as an Ensign and was later advanced to commander of a PT boat operating in the Mediterranean theater. When he returned in 1946, he was appointed assistant professor and assistant plant pathologist on the Davis campus, where he worked on diseases of cereals in cooperation with Dr. Houston.

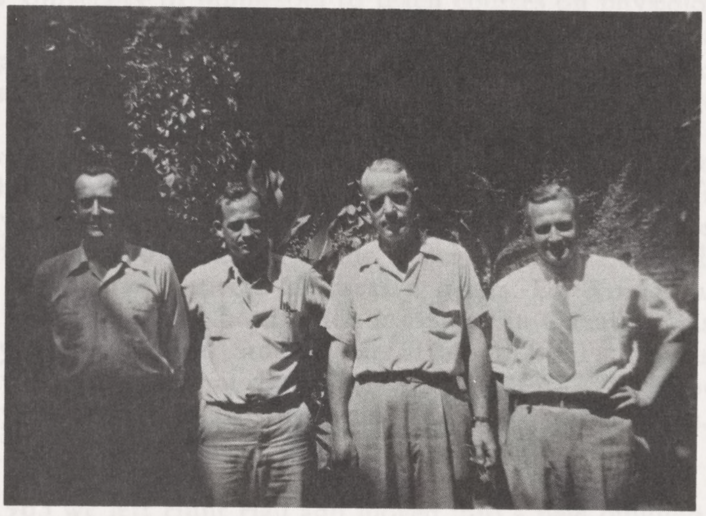
Hasn't he gone on to become president of my Alma Mater, Penn State?

Dr. Oswald was on the Davis faculty from 1946–1954. Upon his return from a sabbatical in the Netherlands in 1954, he transferred to Berkeley to become vice-chairman of plant pathology. He also served as assistant to the president and vice president under Clark Kerr, president of the University of California.

In 1963 Professor Oswald became president of the University of Kentucky and held that position for five years before returning to Berkeley as executive vice president of the University of California. In 1970 he became president of Pennsylvania State University, a position he still holds. He will retire in June 1983.







Above: R.E. Smith, first plant pathologist at the University of California, serving from 1903 to 1944. *Center:* James B. Kendrick, Sr., head of plant pathology at Davis from 1927 to 1960. *Below:* Byron R. Houston, William B. Hewitt, Edward E. Wilson, Lysle D. Leach (left to right), faculty of five of plant pathology at Davis c. 1939. (The fifth faculty member is Dr. Kendrick, pictured in the center photograph.)

Getting back to the history of plant pathology, what effect did World War II have on your department?

In 1942 all instruction on the Davis campus was discontinued and the teaching areas were turned over to the U. S. Army Signal Corps as a training facility. Many of the younger faculty members entered military service but all departments in the College of Agriculture continued their research activities, although much of it was redirected to the food production needs of a nation at war. At the end of World War II plant path was still housed in the old Animal Husbandry Building where we had a limited number of offices and very limited research space. By 1947 there were only six permanent department members.

When did your department outgrow that 1910 building?

In 1948 the University completed a new U-shaped building located directly west of the [Hickey] gymnasium. It was named Hunt Hall after former Dean [Thomas F.] Hunt, and plant pathology was assigned the second floor of the north wing. This gave us a considerable increase in space for offices, teaching facilities and for research. This started a very rapid period of expansion. In anticipation of our new building, Dr. Kendrick recruited Dr. W. Harley English in 1947, who had worked at Oregon State University and with the USDA in Washington. Harley assisted Dr. Wilson with diseases of deciduous fruits. Also in 1948, Dr. George Nyland came from Washington State to take up studies on virus diseases of stone fruits, and the same year Dr. Ray [G.] Grogan came from the University of Wisconsin to work on vegetable crop diseases. In 1953, Joseph Ogawa, who had completed his undergraduate and graduate studies at Davis, was employed to study epidemiology and chemical control of diseases on deciduous fruits. In 1955, Dr. Edward E. Butler came from the University of Minnesota to teach a course in mycology and to conduct research on postharvest diseases of fruits and vegetables. In 1956, Philip Halisky came from Washington State College to work on field crop diseases. He remained in the department until 1964, when he accepted a position at Rutgers University. In 1957, Dr. James E. DeVay came from the University of Minnesota to teach courses on physiology of plant pathogens and plant diseases. In 1959, Dr. Robert N. Campbell came from the University of Wisconsin to work with Dr. Grogan on vegetable disease and Dr. Tom Shalla, who had completed his graduate studies at Davis, was employed to teach a course on virology and to study virus diseases of stone fruits.

After we moved to Hunt Hall in 1948, nine new staff members were employed during the period that Professor Kendrick was the vice-chairman and after 1954 chairman of the plant pathology department. Dr. Kendrick retired in 1960. After I became chairman the first faculty appointment (in 1961) was Dr. Tsune Kosuge, who joined the department to undertake research work on biochemical aspects of diseased plants. Dr. Kosuge had an interesting background in that his B.S. degree had been taken in botany in Colorado, his Master of Science was in plant pathology at Washington State University, and his Ph.D. was taken in biochemistry at the University of California at Berkeley. Also in 1961, Dr. Robert J. Shepherd came from the University of Wisconsin to study virus diseases of field crops. In 1965 he transferred to the University of Arkansas, but returned to Davis in 1967.

In 1963 we left Hunt Hall and moved into a new building that was named Hutchison Hall for Claude B. Hutchison, vice president, Agricultural Sciences. We occupied third and fourth floors, with teaching facilities on the first floor. This gave an opportunity for the addition of several staff members.

You continued to add faculty members in the '60s?

Yes. In 1964 Don Mathre completed his Ph.D. degree at Davis and was employed as plant pathologist on diseases of field crops. Three years later, he decided that he should accept a position at Montana State University. Most of us thought he was making a mistake to transfer from the University of California to Montana State but he was interested in more pioneering types of research and could visualize that with the increased population in California, certain problems would develop which he preferred to avoid. Robert K. Webster, who had completed his degree at Davis, spent a year of postgraduate study with Dr. Richard Nelson at North Carolina State University and at the end of that year we persuaded him to return to Davis (1966) to set up courses and research work relating to genetics of plant pathogens and plant disease.

The last staff appointment before my retirement was Dr. Clarence I. Kado, who completed his doctoral degree at the University of California, Berkeley. He specialized in bacteriology and virology.

Since 1968 five additional faculty members have been appointed in the department: Drs. John M. Duniway in 1970, David G. Gilchrist in 1975, Mary Ann Sall in 1975, James D. MacDonald in 1978 and Richard M. Bostock in 1981.

In addition, four research scientists employed by the USDA are housed with the department and make important contributions to our research program: Dr. Austin C. Goheen, who specializes on diseases of grape vines; Dr. John M. Mircetich, who studies the root diseases of deciduous fruit trees; Dr. William C. Schnathorst, who conducts research on diseases of cotton and other field crops; and Dr. John M. Klisiewice, who works with oil crops.

There have been five retirements from the department: Kendrick in 1960 [he died May 30, 1962], Leach in 1968, Wilson in 1968, Hewitt in 1974, and English in 1978. Dr. Houston suffered a severe stroke and died in 1970 [December 1].

During your first years as department chairman, the planning of Hutchison Hall must have been one of your important considerations.

When I became chairman of the department in 1960, a start had already been made on the plans for Hutchison Hall, and the plan included plant pathology moving to this new building and occupying the third and part of the fourth floors. Dr. Kendrick had put in a good deal of time in this planning, and Dr. Wilson and I also participated in it. Then after Dr. Kendrick's retirement we continued with the plans for this new building. And the move was finally made in 1963. In later years plant pathology has received additional space on the fifth floor, after the Department of Biochemistry moved into Briggs Hall.

Were there other problems that required administrative decisions during the growth years?

I would say that the increased staffing of the department was the activity that required the most time. Dr. Kendrick was primarily responsible for the selection and bringing in of about eight or ten staff members during the period up to 1960.

Were faculty appointments the prerogative of departmental chairmen then? How were they handled when you were chairman?

The chairman was responsible for the recommendation of new staff members to the dean of the college and for biographical information to support the proposal. Senior faculty members participated in the selection of the prospects and in preparation of supporting information. As the department increased in size, special committees prepared the proposals and opinions of all faculty members were considered.

There were also a number of very important research projects that were undertaken or completed during this period. And the department, because of the increased staffing and the very active program on investigation of plant diseases, began to attract large numbers of graduate students. By 1967 fifty-seven graduate students were studying for M.S. or Ph.D. degrees in plant pathology, including several students from other departments such as nematology and vegetable crops.

At the present time, in 1981, we are still housed in Hutchison Hall and the allotted space is filled to capacity with faculty members, graduate students and staff employees.

What about the supporting staff in your department?

In plant pathology we have been very fortunate to have some very efficient women working in the secretarial positions or, as it has been called in more recent years, the administrative assistants. Soon after Dr. Kendrick arrived at Davis in 1927, he employed Miss Ruth Miller as secretary. Later she was married and left the department. The next secretary that I vividly recall was Miss Dorothea Nicholson who married Professor Paul Knowles, professor of agronomy. Dorothea served in a very efficient manner for several years. After World War II Lois Kendrick, the wife of John Kendrick, now vice president of Spreckel Sugar Company, was the departmental secretary while her husband was completing his master's degree in animal husbandry. Dr. J. B. Kendrick, then about fifty-three years old, had a lot of fun introducing this very young and attractive secretary as Mrs. Kendrick to his office visitors—and then to leave them guessing.

About the time the department moved into Hunt Hall in 1948, Mrs. Irene Bailey, the wife of Professor Stanley Bailey, an entomologist, headed our office. Then in 1951 Dr. Kendrick employed Mrs. Erna Thompson, wife of Orville Thompson, later professor of applied behavioral science. Erna served as secretary for one year while Orville was completing his master's degree, and then they both went to Cornell University where Orville completed all the requirements for his Ph.D. degree. In 1954 they returned to Davis where Orville accepted a position on the faculty, and Erna again became the secretary and later administrative assistant in the plant pathology department. That position she still holds.

She has witnessed a few changes over the years, I suspect.

Erna Thompson has been extremely efficient in carrying out the duties of administrative assistant for the past thirty years. During my eight-year term as chairman of plant pathology, it would have been impossible for me to fulfill all the requirements of the office without the outstanding assistance and guidance provided by Mrs. Thompson. I'm sure that other chairmen will agree with me, and I feel sorry for the first chairman who serves in that capacity without the benefit of her experience and her unusual capacity to turn out reports in record time.

And you wrote a lot of reports judging from your lengthy bibliography.

The secretaries in the office do all of the typing of correspondence, papers that are being prepared for publication, and other reports such as budgets, promotion recommendations, grant requests and annual reports.

What about laboratory assistants? That's an important support group in a department such as plant pathology.

Yes, the department's reputation is built to a very large extent on their research publications and on their undergraduate and graduate teaching, but much of this would be very difficult to do without the support groups, both in the department and in other areas. For laboratory work, there is a need for a great deal of media for culturing organisms and a group in what is called a "kitchen area" are responsible for washing and sterilizing glassware and for the preparation of special media. For greenhouse experiments we have another group of full-time employees who take care of the greenhouse and do the watering and sterilizing of soil and all the other activities that are necessary to keep greenhouse experiments operating. Then for those who work in the fields and orchards we have a full-time crew who work in the experimental field plots and assist in the collection of observations and in the harvesting of the results of the experiments.

The successes as well as the failures?

Yes, that's right. In the research areas we have a large number of full-time laboratory technicians who carry out much of the experimental work under the direction of faculty members. Then within the department are a large number of research assistants and other graduate students who are working on their own individual research problems, but under the supervision or with the advice of faculty members who are working in this area. In a large department it requires many more people in what would be termed as supporting areas than are actually involved as the faculty members, extension specialists or other staff positions.

That probably wasn't always the case in the old days. Did you do many of those jobs yourself?

This is true. Many of these—some quite routine jobs—were done by the faculty or experiment station personnel. The first assistance that we had was the employment of undergraduate students on a part-time basis. It was many years before we began to have the means of hiring specialized laboratory technicians and the support of large numbers of graduate students working in the research areas.

RELATIONS WITH AGRICULTURAL EXTENSION PROGRAMS

What was the relationship of members of your department to the University's long-standing program of agricultural extension, or cooperative extension as it is now called?

Most of us have enjoyed excellent relations with farm advisors in the counties. The final step in testing most research findings is to conduct trials in growers' fields in cooperation with farm advisors. The essential link between the researcher and the farm advisor is the extension specialist, a well trained plant pathologist, housed with the department and supplying the latest information to extension personnel. In recent years much of the applied research is being conducted by extension plant pathologists.

Before 1950, C. E. Scott was the only extension plant pathologist in California. He was housed on the Berkeley campus but worked with pathologists on four campuses and with many types of crops.

Upon his retirement, extension plant pathologists were appointed at Berkeley, Davis and Riverside. The first extension plant pathologist on the Davis campus was Dr. Dennis H. Hall, appointed September 1, 1955 to work with diseases of vegetables and field crops. Two months later Dr. Harry J. O'Reilly was located at Davis to serve as extension specialist in the area of deciduous fruits and nuts. In 1969, he accepted a position in British Columbia and he was replaced in 1970 by Dr. William J. Moller, an Australian who had completed his Ph.D. at Davis. He held the position of extension specialist on diseases of fruit and nut trees until his death in 1981. So far (1982) he has not been replaced.

You, yourself, have done a good deal of cooperative work in recent years, particularly with Jack Hills, even though he's not an extension specialist in your department. Was this common for people to work in interdepartmental relationships?

Yes, I think it has always been common for interdepartmental cooperation to bring the expertise of people in different areas to bear on a single problem.

Jack Hills had his graduate training, both for the Master's and Ph.D. degrees, in plant pathology. So even though he is an extension specialist in agronomy, his emphasis has always been on plant disease problems. So it was very easy to work with him in this area. Previously I had worked with people in soils and irrigation on certain problems. I have worked with Harry Lange in entomology on the problem of combining seed treatments with fungicides and with insecticides so that more than one problem could be solved with a single series of treatments. I have worked with people in ag engineering with reference to use of fungicides on processed sugar beet seed and on the development of a spray type seed treater which has been widely used in the western states. In addition, I've worked with people in vegetable crops and certain other departments, so I've had a rather wide series of cooperative activities with people in other departments. For certain kinds of problems I think that this is essential to bring people with different experience or interest together to solve the whole problem. Very often by working with people in other departments you get a much broader approach and a better chance of success in solving the problem.

Yes, you'll be discussing these research projects with Dr. Hills in subsequent interviews.

OUTSTANDING RESEARCH ACCOMPLISHMENTS

Lysle, would you name some of the outstanding research contributions made by your faculty colleagues over the years.

One problem that I think attracted a great deal of attention to research on this campus was work conducted by Dr. Hewitt and by Dr. Houston. Dr. Hewitt was studying the Pierce's disease of grapes which was destroying thousands of vines in several of the grape producing areas. He was able to show that this disease was transmitted from infected to healthy grape vines by large leafhoppers referred to as sharpshooters. Studies were undertaken in cooperation with entomologists to develop methods of protecting the grapes from this infection. About the same time Dr. Houston was working with a disease of alfalfa and he found,strangely enough, that the same kind of leafhoppers served to transmit the dwarf disease of alfalfa from infected to healthy plants. It eventually developed that these two extremely important diseases on totally unrelated crops were caused by the same pathogens and transmitted by the same insects. This I believe had not been observed in any other diseases up to this time.

A few years later, Dr. Hewitt studied some virus diseases of grapevines that were found to live over in the soil. The mystery was how this soil-borne virus could be transmitted from diseased to healthy vines. Working with Dr. Dewey Raski in the nematology department, and with Dr. Austin Goheen of the USDA, it was determined that nematodes were serving as vectors of the disease; that is, the nematodes were feeding on diseased plants and carrying the virus to the roots of healthy plants and in that way spreading the disease. With this information, it was possible to develop a method of fumigating the soil to kill the nematodes and thus prevent the spread of the disease. This was the first record of nematodes serving as vectors of a virus disease and is considered one of the outstanding discoveries in plant pathology in recent years.

A few years later, a disease began to affect barley, particularly during very wet years. At first it was believed that it was due to excessive moisture in the fields during the spring months, but Dr. Oswald and Dr. Houston were able to show that this yellowing of barley plants was caused by a virus called yellow dwarf, that was spread by aphid populations favored by cool moist conditions. The same disease occurred in Europe and, when Dr. Oswald was on sabbatical leave in Holland in 1954, he was able to demonstrate that the disease in Europe was the same as that in the United States, also transmitted by the same aphids.

More recently this disease has been very destructive in some parts of South America, particularly in Chile. So this finding concerning the nature of the yellow dwarf disease has been of international importance.

In 1960 Dr. Ray Grogan and Dr. Robert Campbell were working with an important lettuce disease in the Salinas Valley called Big Vein. They determined that this was a virus disease and they made very intensive studies to determine how it was transmitted from one plant to another. They found that it persisted in the soil and that plants grown in soil infested by this virus would contract the disease. They also found that this disease was nearly always associated with a fungus growing in the roots of the lettuce. By some very careful work, it was finally shown that the fungus was the carrier of the virus and this constituted the first proof that a fungus could serve as the vector of a plant virus. For this research they were given the Campbell Award in 1962 for the most outstanding achievement in the field of plant science. These are just a few examples of research in the department that received international acclaim.

CHANGING EMPHASIS OF TEACHING AND RESEARCH SINCE 1929

From your present perspective, how has the emphasis of teaching and research changed since 1929?

While we were located in the old animal science building (1929–1948), most of the staff members were trained in general plant pathology and the research was largely a matter of problem solving. There were many demands by different phases of agriculture for solution of plant disease problems and most of our activities were in that direction. After we moved into Hunt Hall [in 1948], the staff was enlarged and became more specialized in certain areas of biological science. The philosophy of the department during this period was that each faculty member would teach and conduct research in the area of his specialty, but would also assume responsibility for problem-solving with reference to certain commodity groups. With the expansion of the staff, and particularly the increase in teaching duties associated with development of a large graduate student group, there was a need for more specialization among faculty members and for more sophisticated equipment. These expanded activities created a need for many more laboratory technicians. These demands could not be provided from regular University funds but were obtained largely from federal grants or from research grants from commodity groups. Some staff members continued to concentrate on problem-solving research, but others tended much more strongly toward the development of basic information in the science of plant pathology.

Budgeting for this expanding research program must have been a major undertaking.

Yes, it was, because we had very few budgeted research assistantships available for students and the great majority of the support had to come from special sources of funding. Some of these were funds provided from agricultural production areas or in special problems that were under investigation. A great many of them came in the association with grant funds, primarily from federal sources such as the National Institute of Health, the National Science Foundation, or from the U. S. Department of Agriculture.

Were those funds usually granted for a specific problem that had been identified?

Yes, most of the outside funds came for very specific projects. The grants from the National Institute of Health or the National Science Foundation were primarily to support basic research and they usually carried with it considerable sums of money available for purchase of highly specialized or sophisticated equipment. Use of these grant funds was not directly related to solution of economic problems involving plant disease. But where the funds came from commodity groups or production areas, then the funds were directed primarily to the solution of problems which might include both basic and applied research, but was intended primarily to solve certain problems.

The University has been criticized in recent years for receiving these funds from special interest groups. Do you feel that kind of criticism is valid?

I think that the availability of these grant funds did change the direction of our emphasis within the department, but it also provided the opportunity for people to work with some very important basic problems and to provide the assistance that was necessary for that because we probably could not have secured funds from any other source to carry out many of the research studies that were done. So it had some very important additions to our program, but did have the effect of tending to divert the main attention away from straight solution of problems and direct it more toward the expansion of information and development of new techniques and new approaches to research, many of which ultimately prove to be of great value.

In the plant pathology department, has a relative balance between research activities and both graduate and undergraduate teaching been maintained through the years?

In my opinion the plant pathology department at Davis has maintained an excellent balance between teaching, research and public service. Some of our best teachers have always been assigned to teach our undergraduate courses and over the years many students from these courses have decided to continue into graduate studies in plant pathology.

Several of our faculty members have received national awards and citations for their research contributions. [Postcript: Our highest award was received in 1982 when the National Academy of Science ranked the Department of Plant Pathology at UC Davis first in the United States for graduate training in plant pathology.]

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PLANT DISEASE INSTRUCTION AT DAVIS

You have a reputation of having played an important role in the department's teaching program. I'd like to have you talk about your teaching methods, your purposes, etc.

Should I discuss the first plant pathology courses that were taught?

Please.

EARLY CLASSES AT THE UNIVERSITY FARM

I believe I've already mentioned that at Davis the first course in plant pathology was taught by Stanley Rogers. In 1912 he was transferred to Davis and housed in the truck crops department. He was given the title of instructor in plant pathology and in 1914 and 1915 taught Plant Pathology 124, a two unit practice course for undergraduate students. Until 1929, degree students in plant sciences at Davis had to transfer to Berkeley to take Plant Pathology 120 and a few other required courses, but in the spring of 1929 Professor James T. Barrett commuted from Berkeley to teach Plant Pathology 120 at Davis with the assistance of Dr. Kendrick and William C. Snyder, then a graduate student at Davis.

EXPANDING NON-DEGREE AND UNDERGRADUATE CURRICULUM

At the time I came, there was no plant pathology course offered for the two-year, or non-degree students at Davis. So whatever plant pathology they were introduced to came in some of the production courses in the crops departments, which then consisted of truck crops, pomology, viticulture and agronomy.

NON-DEGREE AND UNDERGRADUATE COURSES

Dr. Kendrick suggested that I initiate a course for the two-year students. This was done first in the spring of 1932 and I was given the teaching title of instructor. The course Plant Pathology 01 was very similar to Plant Pathology 120 except that because the students had few preparatory courses, I had to minimize the more technical aspects and concentrate primarily on the recognition of plant diseases, the life cycles and methods of control. I taught this course for a number of years and also assisted Professor Barrett in the laboratory in the Plant Pathology 120 course.

In 1932 Professor Barrett became ill during the spring semester and required an operation. He asked me to take over the 120 course, to complete the lecture and laboratory material and to give the final examination. The next year, he asked the chairman of the department to turn over the 120 course to me as he no longer wished to commute from Berkeley. So 1933 was the first year I had charge of the Plant Pathology 120 course and, following this, I was granted the title of assistant professor of plant pathology. Mine was the first professorship given in the Department of Plant Pathology at Davis.

Because I grew up in the Middlewest, I knew almost nothing about citrus diseases. Several of my students lived in southern California and wanted information on this subject. I therefore asked Dr. Kendrick if I could transfer to the Citrus Experiment Station at Riverside for about four weeks during the summer of 1932. This gave me an opportunity to talk with some of the best informed citrus pathologists in the country, including Dr. Howard S. Fawcett, chairman of the department and the author of the leading textbook on citrus diseases; Dr. Leo J. Klotz, who had worked with Dr. Fawcett for many years, with Dr. Pierre Miller and with Dr. Donald Bliss. I learned to recognize most of the common citrus diseases and observed their research studies aimed at control. We also took many field trips which gave me a chance to see the diseases in citrus orchards and to observe methods of controlling fruit rots in packing sheds. I have always been indebted to the staff at Riverside who prepared me to teach these subjects at Davis.

Weren't the Olympics being held in Los Angeles during that summer?

Yes, 1932 was the year of the first Olympics held in Los Angeles. My folks were living in Long Beach then, so when I finished this period of study at Riverside, we spent a few days at Long Beach. This coincided with the period of the Olympics so we were able to attend some of the sessions at the Coliseum.

That must have been a memorable experience.

It was for me because I had attempted to coach track in high school some years before and the performances that I saw in the Olympics were so far above anything that I had ever experienced before that it was really quite a thrill to attend.

After I returned to Davis I continued to teach both the non-degree course [01] and the degree course [120] for several years until Dr. Hewitt took over the non-degree course in 1940.

What other courses were included in the plant pathology curriculum?

In 1933, the department first offered special problem courses for undergraduate and graduate students. The graduate students were mainly from other departments, but one student, Harold Reynolds, was enrolled as a graduate student in plant pathology. Later he transferred to another school and completed his Ph.D. degree in nematology. Then, in 1935, Dr. Kendrick organized a course on diseases of vegetable and field crops. Dr. Gardner offered a similar course at Berkeley and they collaborated in the teaching of this subject on the two campuses in alternate years. In 1939, Dr. Wilson offered a similar course on diseases of fruit and vine crops.

With these added teaching assignments, were members of the department given academic promotions in the '30's?

Yes. I was appointed assistant professor in 1934, advanced to associate professor in 1940 and to full professor in 1947. Dr. Kendrick was granted the title of assistant professor in 1936 and was advanced to associate professor in 1938. Dr. Wilson was appointed assistant professor in 1940, associate professor in 1943 and was advanced to full professor in 1947. We all had corresponding advancements as plant pathologists in the Agricultural Experiment Station.

As time went on, and with an enlarged faculty, we were able to offer more courses. In 1947, Dr. English organized a course on phytopathogenic fungi, that is, fungi that cause plant diseases. This course was listed as Plant Pathology 124 and was our first course in mycology for graduate students. Later this course was taught by Dr. E. E. Butler.

In 1949, Dr. Hewitt first taught a course, Plant Pathology 122, covering methods in plant pathology.

I continued to teach Plant Pathology 120 with the assistance of Professor B. R. Houston until 1951, when he took over the course and I organized a new course numbered Plant Pathology 128, an advanced plant pathology course for graduate students only. The number was later changed to 215, then to 235.

By this time, we had accepted a number of graduate students in the plant pathology department and there was a need for more specialized types of training. A number of new faculty members were brought into the department and they were given the responsibility for teaching in the specialized areas.

FIRST GRADUATE STUDENT IN 1937, FIFTY-SEVEN IN 1967

Who were the first graduate students in plant pathology at Davis?

The first plant pathology graduate student at Davis who completed his Ph.D. degree was William B. Hewitt, in 1937. He was followed by Byron R. Houston in 1939, John W. Oswald in 1942, Howard Miller in 1948, Klayton Nelson in 1949 and Richard Converse in 1951. All of these graduate students were required to spend one year of graduate studies on the Berkeley campus and their Ph.D. degrees were conferred at Berkeley.

Carl Nichols was our first graduate student who completed all of his work at Davis where his Ph.D. was also granted [in 1952].

Let me ask, how does the curriculum in plant pathology today differ, if it does, from when you came in 1928?

Well, in the early period, up to 1940, most of the research in the department was related to identification of diseases, studying life cycles and control measures. There were many very urgent problems available and most of the staff was involved in problem solving investigation. At that time graduate students were usually assigned to some of these problems, and their specialized training was largely taken in other departments such as botany, chemistry and bacteriology. By 1950 we had eight graduate students in the department at Davis, and by 1960 this had grown to thirty.

There was a very great need for more specialized training than we had been able to provide previously. This accounted in part for the very rapid increase of staff members during this period. The number of graduate students continued to grow rapidly until 1967, when there were fifty-seven graduate students in plant pathology at Davis. At that time, this was the largest number in any department of plant pathology in the United States or in the world, for that matter.

What attracted them here?

I think they were attracted, first, by the national and international reputation of our faculty members who were prepared to provide training in specialized areas of the science and also by the greatly improved facilities that we had in the department during this period.

Lab facilities?

Yes, particularly laboratory research facilities. Also, graduate students are always influenced by the availability of research assistantships and the opportunity to work with certain faculty members who are developing a national reputation.

After 1967, two things began to happen. Funds for supporting graduate students were less available and there were fewer job opportunities. By 1977, the number of graduate students had been reduced to forty-two, and has remained in the neighborhood of forty to forty-two since that time.

In the fifty-five years of the existence of the plant pathology department at Davis [1927–1982] we have granted 176 Ph.D. degrees and 140 M.S. degrees.

That's a surprisingly large number.

I was *very* surprised at that total. I knew that the number of graduate students had increased very rapidly during this period, but I had no idea that there had been 176 Ph.D.'s granted. I do know that while I was chairman, the numbers had increased considerably because we were getting more sources of funds that could be used to support graduate students and there were a great many more graduate students wanting to come here for their graduate studies. Some of these students were specializing in nematology or other fields of science, but took their degrees in plant pathology. But the great majority were those who were interested specifically in certain diseases of plants and of their pathogens, and have continued actively in the field of plant pathology.

REFLECTIONS ON TEACHING METHODS, FORMER STUDENTS

Lysle, you've mentioned courses introduced by others up to 1951 when you introduced a course. What courses did you teach from 1931 to 1951?

Only two courses. As I've stated, one was Plant Pathology 01, later numbered 52, the course for two-year or nondegree students. The other was Plant Pathology 120, the introductory course for plant science students.

During the twenty years you taught those two courses, did you utilize any methods of teaching that were more or less unique to you and to your personality?

I tried to visualize what the interests of students coming to the class would be in plant diseases, and what their needs would be after they graduated. Then I attempted to construct the class in such a way that these needs would be fulfilled.

You stated your teaching philosophy in a paper that was published in the "Plant Disease Reporter" dated April 15, 1950 [Vol. 34, No. 4], saying: "In my opinion the content of a course should be determined by the needs of the student and by the probable use that the students will make of the subject matter after their graduation. With this in mind, we first analyze our enrollment and consider the type of student with which we deal."

We had a unique opportunity at Davis to work with fresh plant disease material almost the year round. And we had facilities in the greenhouse for growing plants and for inoculating so that we could demonstrate fresh material in the laboratory for almost every example that was used.

To me, the laboratory was the best teaching opportunity, because here people were working with fresh materials and could much more readily visualize the causal agents, the effects upon the plant, and the approaches that could be used for control. I tried to select examples that would cover all of the types of plant diseases and the most important pathogens, whether they were fungi, bacteria, or virus diseases. I also tried to select the examples that represented some of our most important diseases in California. And, I tried to make them as interesting as possible by bringing in material that would show the importance of the diseases and unusual types of relationships between various pathogens and their host plants.

Lysle, you brought us up to 1951 and the course that you introduced. Would you take the courses from '51 to the present time, if you can, or certainly up to the time of your retirement.

Well, I was serving as dean of students from 1952 to 1958 and during this period, the only course that I taught was the advanced plant pathology course 128, or later numbered 215. I didn't do any more teaching after that, concentrating instead on research and then serving as department chairman until I retired.

Who were some of your outstanding students during those years, both undergraduate and graduate?

In the 120 course that I took over from Dr. Barrett were two outstanding students, who later became faculty members but not in the field of plant pathology. One was Dr. [Maynard A.] Amerine who later became professor of enology and known worldwide for his research on the technology of wine making. Another was Claron Hesse who became professor of pomology, chairman of the department, and director of the Kearney Horticultural Field Station.

How were they outstanding at the time?

Both had made a decision to go into graduate work and to specialize in horticultural fields. As a result, they were quite eager to secure as much information as they could that would be useful to them. And they were exceptionally good students. I can't really claim them as *my* students but they were in that first class I took over.

Bill Hewitt and Byron Houston were two more outstanding students who completed their degrees in plant pathology and became members of the staff, the first staff additions after E. E. Wilson and I came to the department here.

What about those that went elsewhere, were there opportunities for graduate students to get jobs in other universities or in the private industry sector?

During the very early years of the '30's and up through the '40's we had very few graduate students here. We had a number of students who took their plant pathology as an undergraduate at Davis and then went to graduate school in other universities, some to Berkeley, some to Wisconsin and elsewhere. So there are quite a few plant pathologists who had their undergraduate work at Davis. But after about 1950 much larger numbers of students were taking their graduate work in plant pathology at Davis, and many of them have been employed in other universities, with the USDA and with some commercial companies. At the present time one of our graduate students is the chairman of plant pathology at the University of Florida, another at the Virginia Polytechnical Institute, and we've had some very important research workers and teachers in a number of other universities. Some of the graduate students preferred to work with private companies. One, Charlie [Charles J.] Delp, went with DuPont quite a few years ago, and he has been largely instrumental in the development of Benlate, a systemic fungicide that has quite an impact on the control of a number of plant diseases.

What about students from other countries?

I'd say when I first came here there were very few foreign students, but as time went on larger and larger numbers came to California, because of the climate and the type of crops that are grown and the educational opportunities here. By 1965 graduate students from seventeen foreign countries were studying for M.A. or Ph.D. degrees in plant pathology at Davis.

Were they sent on scholarships mostly?

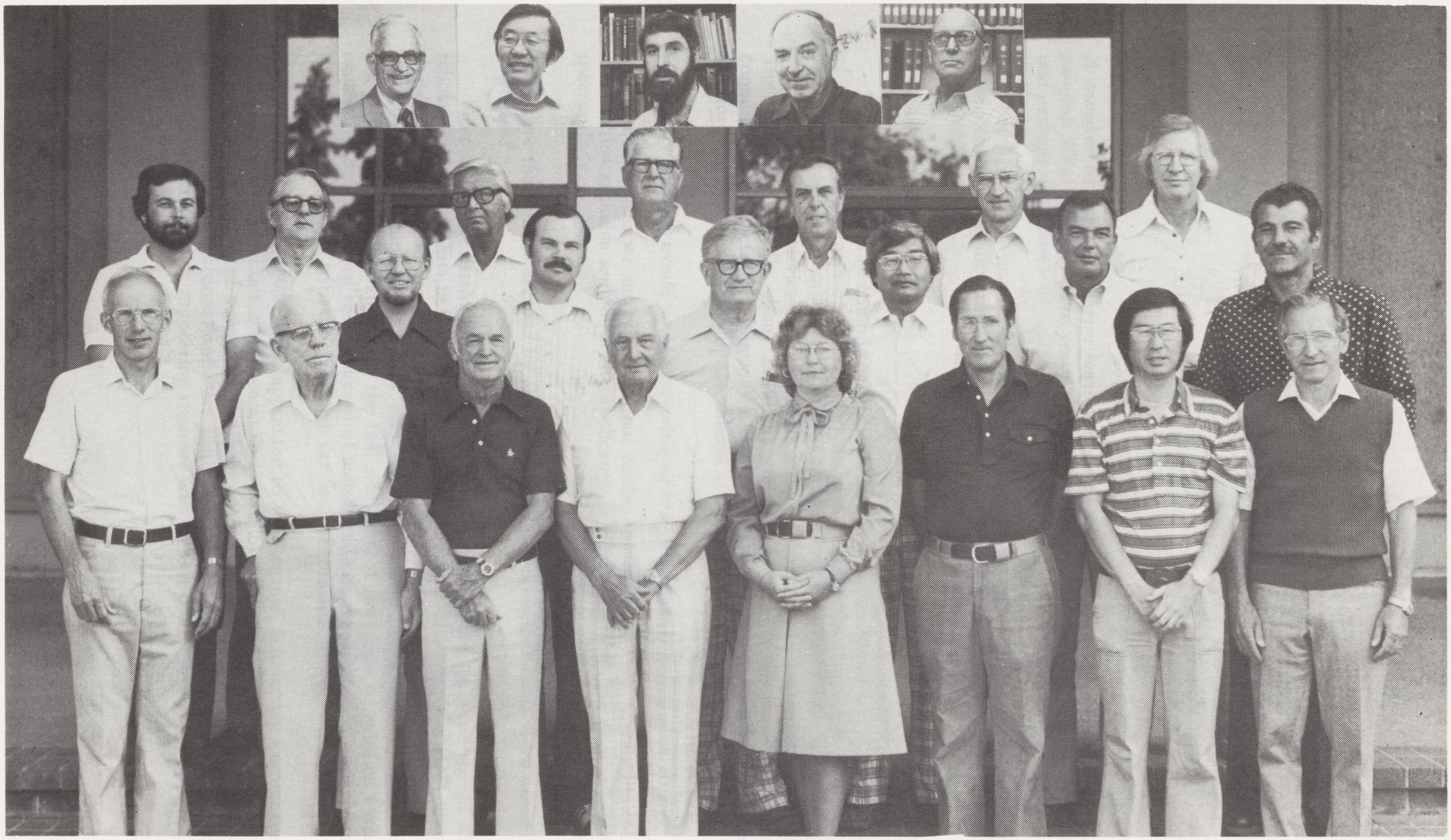
Most of them came on scholarships from their own governments or under some of the international exchange programs. For example, the Rockefeller Foundation sent a number of students here, as did Ford Foundation, and the AID program.

Did you have any women in your classes?

As I recall the first few classes there were all men, but a few years later we would have one or two women students in the class, but not any large numbers until many years later.

Are there opportunities for women in plant pathology today?

There are many more opportunities today than there were some years ago because quite a few companies are hiring women either in their research work or in their field activities. The Extension Service at one time employed women only in their homemaking departments, but now several counties have women in regular farm advisor capacities working with crops or with crop diseases.



Faculty - DEPARTMENT OF PLANT PATHOLOGY, UNIVERSITY OF CALIFORNIA, DAVIS - 1979

Top: William B. Hewitt, Tsune Kosuge, John M. Duniway, Dennis H. Hall, Raymond G. Grogan
Back Row: Edward E. Butler, Bert Lear, George Nyland, William C. Schnathorst, John M. Klisiewicz, Tom A. Shalla
Middle: David G. Gilchrist, William J. Moller, James D. MacDonald, Austin C. Goheen, Joseph M. Ogawa, Robert K. Webster, Srecko M. Mircetich
Front: Robert N. Campbell, Edward E. Wilson, Harley English, Lysle D. Leach, Mary Ann Sall, Robert J. Shepherd, Clarence I. Kado, James E. DeVay

· VIII ·

FACULTY – STUDENT RELATIONS

A number of your colleagues have mentioned your special interest in students on the Davis campus. What activities were you particularly involved in during the years before World War II, and perhaps since then?

I was involved quite early in the undergraduate scholarship committee and I was also appointed as the faculty athletic representative—or the faculty representative to the Far Western Conference.

STUDENT SCHOLARSHIPS

On the scholarship committee—I should say that before World War II the Northern Section of the Academic Senate on the Berkeley campus included the assignment of a few scholarships that were available to students on the Davis campus. Professor Ed [Edwin C.] Voorhies each year came to Davis to interview the local applicants. One year Ed asked me to sit in on the interviews. The following year [in about 1939] he arranged for me to be appointed to the Berkeley committee, and thereafter I interviewed the Davis applicants and made recommendations to the Northern Section Scholarship Committee at Berkeley.

How many students would you be interviewing in a year's time?

The first year we interviewed about twelve or fourteen. There were a limited number of scholarships available and very few applicants, but over the years the numbers increased quite spectacularly.

How were these funds accrued by the University? Did donations come from private individuals or groups?

Most of the scholarships were the result of donations by individuals or groups who provided scholarships to students in certain colleges or certain areas of study. The [UC] alumni scholarships were available to any student on any of the campuses. About three years after I started on the scholarship committee [in 1947], they decided to set up a separate scholarship committee on the Davis campus. I was appointed the first chairman. A few years later, when I was appointed dean of students, I asked to be relieved of membership on the scholarship committee, but I continued to work closely with them through the dean of students' office.

After I retired in 1968 I was appointed to the Regents' Scholarship Committee and served in that capacity for about three years. So I've followed the scholarship activities on the Davis campus over a period of years and I've been instrumental in helping to set up several of the scholarships where the donations were made by agricultural organizations or people who were particularly interested in supporting students who were studying agriculture or home economics.

ATHLETIC PROGRAMS, INTERCOLLEGIATE SPORTS

Lysle, in what ways did your athletic experience in high school carry over to UC Davis?

In 1929, when we moved to Davis, we found a very interesting athletic program in somewhat limited areas. For example, they had a very strong football program and an exceptionally strong boxing program, but in some of the other sports they were just getting actively started.

I'm told that you and your wife have rarely missed an athletic event on the Davis campus over the years—even today. Is that correct?

Yes, I would say that is correct as far as games held on the Davis campus. We have, I think, attended all of the football games and most of the basketball games except in one or two years when we were in foreign countries during the season.

Didn't you even attend a football game in Oregon when you first came to the campus?

Yes, the first year I was at Davis, the Cal Aggies opened the season by playing football against Oregon State University at Corvallis, Oregon. It happened that I was just finishing up my work with strawberries and Dr. Harold Thomas from Berkeley and I were collecting material in the Willamette Valley of Oregon. We found that it was possible to arrange to be in Corvallis on a Saturday afternoon, so I saw the first game of the 1929 season and, in fact, I held one end of the chains on the field. This was the last time that the Cal Aggies played Oregon State.

Was it the first time?

No, it was the second time.

And did they win?

No, the Aggies had a squad of about eighteen football players and all of them played both ways with only a few substitutes. Oregon State had a very large squad and in fact used three platoons, successively, in the game. The score at the end of three quarters was still nothing to nothing, but the Aggie players by this time were so tired going up against different groups of a large university squad that they began to tire, and in fact the linemen were crawling back into their positions after each play. So Oregon State scored three touchdowns and won the game by 18-0—all the scoring in the last quarter.

When did you begin serving as faculty representative on the Far Western Conference?

In 1941, Dean Ryerson appointed me to that intercollegiate post, and I served in that capacity until 1963.

What were your responsibilities?

The first duty was to represent the campus and the chief administrative officer on the campus in the meetings of the faculty representatives of the Far Western Conference, which before the war included the University of the Pacific—then College of the Pacific, Chico State College, Humboldt State College, and Cal Aggies.

Those were the schools that Davis played regularly during athletic competitions?

Yes, we regularly competed with the other conference schools in all sports, but we also played a number of other teams that had formerly been in the Far Western Conference, or later were members. Several schools such as San Jose State, Fresno State, University of Nevada, had withdrawn from the Far Western Conference to play in larger school conferences. So there were only four teams just prior to World War II.

At these meetings the principles under which the conference athletic participation was conducted were a primary responsibility. This included rules for eligibility, proceedings for exchanging information among the conference schools, and in general supervising the conduct of athletic participation. The directors of athletics and coaches had a separate group. They were concerned more with the rules applying to conduct of the games. Whatever decisions they made were then referred to the faculty committee for approval. As a faculty representative on the Far Western Conference, I also served on the executive committee of the associated students [ASCA] and that meant one evening a week devoted to the associated students and their relation to the athletic program.

Were there cases in this conference where schools were offering athletic scholarships?

There was a rule that no grants-in-aid could be given to participating athletes. This had to be rather carefully defined because all students are eligible for academic scholarships and all students are equally eligible for jobs on or off campus which are paid at the prevailing rate. But it was necessary to rather carefully define what kind of jobs should be available to athletes and what inducements might be made to attract them to different schools. Several of the schools were anxious to get into participation with larger schools and were very much inclined to offer some kind of financial assistance. If they were offering athletic grants-in-aid, they would no longer be eligible to participate in the Far Western Conference, and they would then withdraw and either participate as an independent or would join another conference where athletic scholarships were permitted. Nevada, on two occasions, withdrew from the conference and later came back into the conference. Of course, at the present time they are now participating in another conference that does permit scholarships. When the College of the Pacific decided that they were going to offer athletic scholarships, they offered to resign from the Far Western Conference so that they would not violate the Far Western Conference rules.

Because of your interest, you've probably been associated with the P. E. department and the coaching staff for a good many years. Do you feel that the athletic programs at Davis have had a good effect on the students?

I think that Davis has had a very good athletic program, both at the intercollegiate level and also at the intramural level. With a small school located in a small city, it would be practically impossible to collect enough money to support an athletic program such as found in most of the large universities where several hundred thousand dollars or a few million may be required as donations in order to provide the athletic scholarships and to maintain their programs. Operating without scholarships makes it difficult to compete with scholarship schools in attracting students who are particularly interested in securing all the financial aid that they can receive. However, with the program at Davis, they have been able to attract a remarkably fine group of student athletes without any offer of financial assistance. The program is very attractive to the student body who support the football program in a very positive manner. The basketball program has not been quite as successful in recent years and attendance is surprisingly low. When we first came to Davis, boxing along with football were the two big sports on campus and almost the entire student body and many of the townspeople would turn out for the boxing matches because Davis at that time was competing successfully with the largest universities on the Pacific coast.

Were they playing big schools like Stanford and Cal?

Yes, in boxing the Cal Aggies competed successfully against Cal, Stanford and UCLA. Some of the strongest competition came from schools like Washington State and Idaho. After a series of dual meets, the Pacific Coast Intercollegiate championships were held each year at Sacramento. There, the best athletes from all of the western schools would compete for the championships. The winners at Sacramento would then go to the national championships, usually held in the East, but on two occasions these were held at Sacramento. So this was a very popular sport and the Davis campus was unusually successful for a small school. Interest declined particularly after World War II, and in the early 1950s boxing was dropped as an intercollegiate sport at Davis, as it was in many other schools about that time.

Who had been the boxing coach during the heyday?

Heine [Henry] Seviere was the coach from about 1927 to 1939. Most people will recognize him as the most successful boxing coach ever on the Davis campus. [Heine Seviere was inducted into the Cal Aggie Hall of Fame in October 1982 in recognition of his very successful coaching career.] He came to the Davis campus in charge of orchard operations and later became superintendent of the experimental farms. He volunteered to coach the boxers and this worked out so well that he continued for about twelve years. During that time he turned out many champions at the PCI's and a few at the national level.

Was he paid for this or did he do it on his own?

I'm not sure. I would assume that he did receive some remuneration, but I can be fairly certain that it was not very much and probably at the beginning he received nothing at all.

What about some of the early football and track coaches at Davis?

Prior to about 1928, there were a series of coaches who were here for a year or two, and then they would move on to another job. But in 1927 and 1928, [C. I.] Toomey, who had played on the so-called Wonder Teams at the University of California at Berkeley, and Woody Wilson, who was an all-American lineman at Texas A&M, came in as the head coach and line coach, respectively. They developed very successful teams in 1928 and 1929. For the next few years there was very little material available and they were not so successful. Vern Hickey came from Santa Rosa as head coach in 1937 and he continued as head coach until about 1949, with the interruption during the war years when no football teams were active on the Davis campus. Vern was from Washington State College and he was invited to, and played in, the East-West Shrine game at New Year's time.

Buildings, playing fields and special events have been named for these people. It's interesting to have a little background. Weren't you involved in the annual Picnic Day track meets now known as the Woody Wilson Relays?

Yes, this was one of the activities that Woody Wilson carried out, being a line coach in football during the fall and a track coach in the spring. For a number of years they invited high schools from all over California who came to Davis on Picnic Day in April each spring and participated in the track meet. There would usually be three classes, based on size of the schools. Each class would run through all of the track events so there were often more than a thousand high schools' track men performing here in one day.

That must have taken a lot of organization.

It was reported to be the largest one-day track meet in the world at that time and it was an annual event for many years [1913–1967]. Eventually this event became too large to handle on Picnic Day, and the high school track meet was discontinued in favor of an invitational college meet. To carry out such a track meet with so many entries required a lot of help, so faculty members were recruited to be judges at the finish, timers and judges of the field events. Perhaps twenty or thirty faculty members each year spent nearly the whole of Picnic Day on the track helping out with this mob of high school track stars.

And you were one of them?

Yes, I served from the time we came to Davis in 1929 until I became dean of students in 1952, and then there were other activities that I had to take care of, so I no longer worked in the track meet.

Was this track meet partially designed to bring prospective students to the campus or was it strictly an athletic event?

At that time the enrollment was very low on the campus and part of the activity was to attract students to come here and to consider attending the University of California at Davis. However, most of the large track teams came from large cities and most of them were not interested in agriculture and therefore relatively few of them actually came to Davis to go to school.

In the period before World War II, besides the gymnasium and the pool that were constructed in 1938, what other recreational facilities were available for students? You spoke of intramural sports . . . .

In about 1932 to 1934, tennis was started on campus and cement tennis courts were built just about where the present Freeborn Hall is located. Soon the program outgrew these courts and the space was needed for other purposes. New tennis courts were built north of what is now Hunt Hall. The baseball fields and new football field was set up north and northeast of the gymnasium. The question came up then about what space would be available for the expanding intramural program. I was on the committee that studied this and we recommended that the area between Howard Way and California Avenue be reserved for intramural purposes. This was done and this large open space is still used for all types of intramural activities and on weekends for soccer or rugby games and various activities.

It's a very well used space, day and night.

If this space had not been reserved for this purpose, I'm sure that buildings would have been built there long before now and this very large space at the north side of the campus bordering Russell Boulevard would not be available.

Were there ever plans to build a stadium or do you feel Davis is large enough to warrant building such a facility?

The present football field is at the northeast corner of the campus, very close to adjacent streets, so there is very limited parking facility except on the streets of the city. The stands, while they've been improved over the years, still are quite inadequate for the crowds. A few years ago they could accommodate about 5,000 at the games. Now by temporary stands and by renting bleachers that are brought in, they can accommodate a little over 11,000 students and spectators. Whenever an attractive game is held here, there will be a demand for anywhere from ten to 15,000 seats, which cannot all be supplied. I'm sure that almost everybody would agree that it would be very desirable to have a new and much enlarged football stadium for the future.

Davis does have a large recreational facility that was built in 1978 that provides a space for athletic sports of all kinds. Were you instrumental in getting that started?

No, I had nothing to do with the planning for the recreational swimming pool or the development of the new Rec Hall, because this was done after I was retired. I was very much interested in it. We contributed to the fund for the construction of Rec Hall and attend a good many events there.

It's named for a previous building that was also called Rec, or Recreation Hall. Could you describe that?

Well, the original Recreation Hall was a wooden structure that was built very early—around 1920. Previously, the students had no place to play basketball and there was no place for large social gatherings. Ed Voorhies, who had previously been a student on the Davis campus, was transferred from Berkeley to teach agricultural economics. On the urging of students and townspeople he went to a member of the legislature and urged him to introduce a bill which would provide about $15,000 to build a gymnasium and recreation hall. The bill passed the legislature and the first that the university administrators knew anything about it was when this appropriation came through to the Davis campus.

Like in some of the early plant pathology appropriations, this request hadn't gone through proper channels.

No, and I think Ed Voorhies indicated that he received a reprimand from the administration for his activities in making it possible. At least the students and townspeople appreciated the fact that at last they were going to have a building that would be suitable for basketball and other indoor activities. The construction crew on the Davis campus built this building and it stood until the 1950s when the old Recreation Hall was torn down and the student bookstore was built on essentially the same location.

Before we go on, one more question about athletics at UC Davis. Weren't you involved in getting the Cal Aggie Athletic Hall of Fame established in recent years?

Yes, I was on the original committee that was appointed to consider the establishment of an athletic hall of fame on this campus. This committee included faculty, students, and some administrative representatives. After studying the problem for a period of time, the committee recommended that a hall of fame be established and recommended certain guiderules to the chancellor for the selection of the inductees to the hall of fame and for the mechanics of carrying it out. This particular program only recognizes intercollegiate athletics.

When the Chancellor [James H.] Meyer approved the establishment of the hall of fame, he appointed a committee to advise the chancellor on the first group of inductees and I was a member of that first selection committee in 1979. At the end of that year, I was appointed as chairman of the selection committee for the second year, and I thought my duties with the hall of fame terminated with the second year of appointment. But it turned out that they asked me to continue on the selection committee for the third and fourth year because there were so few people still active on campus who had been in Davis in the late 1920s and through the '30s and '40s that were familiar with most of the athletes who participated at that time.

This award is usually presented during the annual alumni weekend, isn't it?

Yes. The awards banquet is held on Friday night preceding Alumni Day.

STUDENT GOVERNMENT, FACULTY ADVISORS

In addition to the scholarship and athletic committees, you were also appointed to a Faculty Committee on Student Affairs in 1941. Do you recall what prompted that committee's appointment?

Well, there are always a certain number of student problems, but as the enrollment of students increased from three or 400 back in the late 1920s up to 1,000 or 1,200 in the early '40s, there was naturally an increase in the number of activities and some of these needed to be considered by campus authorities. So Dean Ryerson asked for the appointment of a committee on student affairs. The first committee was appointed in 1941 with Elmer Hughes, who was a professor of animal husbandry, as the chairman and included seven additional faculty members. I was on this original committee. After the World War II, the committee was reactivated in the spring of 1945. Jim [James F.] Wilson who was a professor of animal husbandry, was the chairman and there were four other faculty appointments. I continued on this committee after World War II.

What about the student government? Was there an active student body organization at Davis before the war as well as in the years since then?

Yes, there was a very active associated student organization on the campus. Because it was a small campus with a limited number of people, it was a very close-knit group. I think the student government served a very useful purpose as an organization and spokesman for the students themselves. Before the time that I came here, they had decided to operate the campus under an honor system. This honor system applied to the conduct of examinations in academic courses, whereby the students pledged that they wouldn't either give or receive information from other students during the examination. As a result, examinations were given entirely without proctoring. The instructors would pass out the examination questions and leave the room in nearly all cases and be available for questioning outside of the examination room. So there was a very high degree of responsibility accepted by the students and, to make this work, the students had to agree that if they saw violations, they would report them. This is perhaps the most difficult part of the honor system—to have students agree that they will inform on each other if violations occur. But I would say that most of the students at Davis were conscientious about this. They believed in the honor system and they knew it wouldn't work unless almost every student was willing to carry his part.

Were the students themselves responsible for setting the terms of the honor system and handling any infractions?

Yes. The administration and faculty agreed to let the students conduct hearings and to recommend penalties for violations.

The associated students established a Welfare Council, composed of elected students, to conduct hearings concerning any violations reported by students or by faculty. The council would listen to the charge and then talk with the person involved and try to resolve it, and if necessary would recommend to the chief campus administrator any penalties that should be invoked. I would say that before World War II this program was very effective and was supported by nearly all of the students. The honor system even extended to the care and protection of the facilities or property on the campus, and there was a very high degree of responsibility among the students for maintaining things and not destroying or abusing anything that was for the use of all students.

WORLD WAR II: CLASSES REPLACED BY U. S. ARMY SIGNAL CORPS SCHOOL

World War II brought many changes. Do you happen to recall what happened immediately after the Japanese attack on Pearl Harbor? What was the effect on both the students and the faculty?

Well, the effect was first shock to think that something of this kind had occurred. The second thought was the concern as to what effect this would have on the operation of the University and particularly to the students and former students who would be directly involved in the hostilities. Classes were continued through the summer session of 1942 to provide courses for students who were nearing their graduation. Every effort was made to assist these students in getting all of the courses they needed before the fall of 1942, because by that time nearly all of the able-bodied men of that age were being inducted into military service. The last course I taught was during the summer of 1942, and after that all courses were discontinued and the entire teaching facilities of the campus were turned over to the U. S. Army Signal Corps.

Were there any problems with the military government coming on to the campus? Research facilities, I understand, continued to operate.

Yes, the research activities of most of the departments continued. Most of the younger members of the teaching departments were associated with some of the military activities, either in training or in special services. I don't remember that there were any frictions between the military administration and the campus authorities because it was just understood that these teaching areas were to be used by the military. After they had been here a while, the officers particularly became associated with organizations like the faculty club and participated in the general campus activities.

Where was their main headquarters?

I think they used part of the Library-Administration Building that was built in 1938 and is now part of Shields Library. They also occupied several offices in the gymnasium.

Now, was the Navy also on this campus?

No, the Navy was on a number of other college campuses. I remember after the Signal Corps had selected the Davis campus, a group of Navy officers looked over our gymnasium, our swimming pool and other facilities. One made the comment that "here is all this, and we took St. Mary's."

What construction occurred during the war years by the Signal Corps, if any?

In some of the universities where military installations were located, the government built permanent buildings or facilities that were used during the war and then were left for the universities afterward. Some of these were housing units and others were for instruction or recreation. However, the Signal Corps trained their people at Davis but the only buildings that they actually constructed were a number of one-story wooden buildings that were covered on the outside with tar paper and had no permanent value after the military use was completed.

So the campus didn't really benefit as they did elsewhere?

Very little. In fact, the Davis campus was the only one of the University of California system that was turned over to a military unit.

POSTWAR WORLD WAR II ENROLLMENT AND STUDENT HOUSING

What about student housing, on and off the campus?

Before World War II, most of the male students on campus were housed in North and West Halls, or off campus in fraternities or in private homes. Women students were housed in South Hall. When the campus reopened at the end of the war in 1945, there had been no improvement in the living facilities, but there was a sudden surge of enrollment, consisting of former students who were returning, and many new students who wanted to attend the University at Davis under the GI Bill. Only a few hundred [380] came to the campus in the fall and spring semester of 1945 and '46, but in the fall of 1946 the big wave of enrollment occurred with about 1,600 students enrolled. This represented between a three- and a four-fold increase over the enrollment of the previous spring and the campus was completely unprepared for housing of this number of students.

These postwar students were quite a bit older than the average college student. Many of them had been officers and had very strong opinions. To meet their housing demands, the University put extra beds into the old Rec Hall, which had been replaced by a new gymnasium, and packed beds into all of the other halls to their full capacity. Some groups became very critical of the University because they had not provided enough housing and demanded that abandoned army barracks be moved onto the campus and fitted for student housing, as had been done on many other college campuses.

Was the faculty involved in pressing the adminstration for more housing? In his memoir, Dean Ryerson has stated that he was an avid supporter, but against some opposition in the University.

The faculty was very anxious to see housing for the students increase because they were conducting classes for these students who had no suitable places to live. Even when they used double decker and even triple decker beds in Recreation Hall, the students still had little or no place to study. And when the administration did not appear to move very rapidly to secure surplus barracks, a group of students from West Hall finally approached the administration and asked very strongly for an improvement of student housing. Ira Smith, the campus business manager, was then in charge of housing and tried to explain to them why the University could not meet their demands immediately. His final response was that the University would do what it could, but if that was not satisfactory to the students, the University had no alternative but to limit enrollment. This was the last thing the students wanted and they became very angry with this response. The leader of the protesting group was Hank Sciaroni, a veteran of the Air Force who had been shot down over Italy. The group decided to take their protest to the highest levels of the administration. Hank came to me because I was currently chairman of the student affairs committee and told me that they were planning to request an interview with President Robert Gordon Sproul and to demand that improvements be made in the housing for veterans on the Davis campus. I did not attempt to dissuade them, but I did urge them to make their request through channels.

Did they accept your advice?

They prepared a written request for an interview and delivered it to the campus administrator, Assistant Dean Ryerson,and he, in turn, forwarded it to President Sproul who promptly granted their request for an interview in his office at Berkeley. Upon returning to the campus, the students told me that President Sproul had been very gracious, had listened to their presentation, and expressed sympathy with the problem. But at the end of the interview, the president had stated exactly as Ira Smith had said, "The University will do everything it can, but if that is not satisfactory to the students, we have no alternative but to limit enrollment." The students realized then that Ira Smith had talked to President Sproul and lost all hope of a quick improvement. The student reaction was to write a scathing editorial criticizing the administration's handling of veterans' housing. This was printed in the *California Aggie* over the signature of the editor, Bill Allewelt, now president of Tri-Valley Growers, one of the largest canning associations in California. To make things more emphatic, Hank Sciaroni had several hundred copies printed and sent one to each member of the Board of Regents and to each Senator and Assemblyman in the state legislature. Before long, the University undertook a crash program to bring surplus army barracks to the campus.

Whether this action was the result of the students' pressure or was the normal building plans of the University is not clear. In any case, three surplus army barracks were installed on the east side of California Avenue, south of Russell Boulevard. They were named Ash, Birch and Cedar and were commonly called ABC Halls. Also, a number of surplus army barracks were installed south of First Street and east of B Street. This group of buildings was named Aggie Villa and was used for married students, while the ABC Halls were for single students.

These served as campus housing for a generation of ex-GI's and other college students. Hank Sciaroni told me soon thereafter that "I think the administration is so angry about the editorial and the pressure we put on them that I don't think they will let me graduate." Of course he did graduate and was then amazed when he was offered a position with the Agricultural Extension Service. He proved to be one of our most effective farm advisors and still serves as County Director of Extension in San Mateo County.

A subsequent article in the California Aggie*—the University's response to the student criticism—referred to the fact that the UC Engineers had dubbed the whole process of obtaining surplus housing "a mess of frozen alphabet soup," because they had to deal with so many government agencies.*

I'm sure that was true, but the fact that other schools had been able to secure temporary housing from military sources made the people locally wonder why it was so difficult for the University of California to get geared up in the same way. Part of the problem, of course, was that the statewide administration was dealing with a number of campuses. They were no more concerned about the Davis situation than they were on any of the other campuses. But the fact remains that the Davis campus was the only one that was closed during the war, and where other campuses had gone ahead in the development particularly of student housing, the Davis campus was no better off than it was in 1942 when the campus closed down. Yet we had probably the greatest percentage increase of enrollment of any of them. So there was this very violent reaction on the part of some of the students to the delays in the campus and statewide administration in solving this problem.

Housing was not only a problem for students, it was a problem for new faculty members who found very limited housing available in the city of Davis.

That's certainly true. A few years later [in 1951], the University undertook a program for construction of modern residence halls, and Hughes and Beckett Halls were built near the north edge of the campus.

Would you talk a little about the fraternities and sororities?

At the time I was in the Dean of Students Office there were no sororities on campus. Several times representatives had come to Mrs. Regan and had indicated that they would like to establish sororities on this campus, but when this matter was taken to the women's association, invariably it was overwhelmingly voted down. At that time nearly all girls were living in the residence halls and they had a very close organization. They felt that bringing sororities onto the campus would be divisive of the woman student body and very few were in favor of it. In the early 1970s, when the enrollment had increased considerably, there was a demand to bring sororities on campus and at that time a decision was made to open it up to sororities. At the present time, around twelve or fourteen are operating on the Davis campus.

Fraternities had existed almost from the beginning of the campus. Most of them were locals except for AGR [Alpha Gamma Rho] which being an agricultural fraternity came onto this campus quite early. Around 1950, when the College of Letters and Science was being added to the Davis campus, a number of the locals decided to petition national fraternities. Several strong fraternities came to the campus at that time. And since then a number of nationals have installed chapters on the Davis campus, about ten or twelve at this time.

In general, the fraternities here were rather low pressure, not very highly social. It was first of all a place to live, and secondly the association with a small group of people which appealed to a certain segment of the campus population.

PLANT PATHOLOGIST BECOMES DEAN OF STUDENTS

About the time the first postwar dormitories were built your involvement with student affairs increased considerably. Will you tell about your appointment as Dean of Students and your experience in that position?

Experiences! Well, in early 1951, Dean Hutchison came to my office, which was a little surprising because usually Dean Hutchison did not go to individual offices but rather asked people to come to his in Berkeley or to the administration building at Davis when he was up for a day. In this case, he told me that they had decided to establish an office of the dean of students on the Davis campus and asked me if I would accept the position as dean of students on a half-time basis. He said that they were very anxious to appoint a faculty member who had been actively working with students for a number of years and they did not wish to appoint a professionally trained dean of students at this time, because they did not feel it would be readily accepted by the faculty members. He said this was not a life sentence but he would not want me to take the position for only a year or two. If, after a few years, I wanted to go back to plant pathology, they would arrange for a change. I then told him that I was negotiating about an appointment under the Marshall Plan to go to Denmark and to Ireland and that I would not return in time for the fall semester of 1951. He said this would be no problem as they would merely delay establishing the dean of students office until the fall of 1952. With that assurance, I indicated my willingness to attempt the establishment of the Office of Dean of Students and to serve in that capacity for a few years.

Was Dean Ryerson in on these negotiations, or did these conversations occur while he was overseas?

It is my impression that Dean Ryerson was still very active with some of the organizations in the South Pacific. This was just at the time when they were planning some changes in the administration at Davis and Dean Hutchison took a very active part in this. I'm sure that he consulted with a number of faculty but I have no information on what kind of consultation occurred with Dean Ryerson or with other campus administrators.

How much of your time was devoted to the dean of students office once you accepted the position?

It was supposed to be a half-time appointment. When I asked about research and teaching Dean Hutchison indicated that in his opinion I would still be able to continue some teaching activities, but he thought it would be very difficult to maintain a full research program and spend the time that would be necessary in the dean of students office.

In practice, it turned out that somewhat more than half-time was required in the dean of students office. And it was also impossible to maintain a schedule of a half a day in one place and a half a day in the other because urgent matters would come up. In the midst of something I was trying to do in plant pathology, I would be called to the administrative offices or the dean of students office to take care of some other matters. I used to say that half of my time was in plant pathology and half of it was in the dean of students office, and another half was running back and forth between them.

Where was the dean of students office located?

It was in the Library-Administration Building, on the first floor of what is now called Shields Library near the northeast entrance.

Wasn't the admissions office nearby?

The admissions office was on the same floor but farther west, near the west end of the building. Ira Smith, the business manager and comptroller, was located on the same floor but to the east or to the left as you entered that building. The provost, later the chancellor, was located on the second floor at the east end of that building. The library then occupied the west two-thirds of the second floor.

One of the main reasons for establishing an Office of Dean of Students was that the College of Letters and Science was opened in the fall of 1951. However, the enrollment during the first two years was much lower than was anticipated. The total UC Davis enrollment in 1951 was about 1,400 and grew very slowly during the first two or three years. Mrs. Susan [Cobb] Regan, wife of Professor Bill [William B.] Regan in animal husbandry, had served as advisor for women and she was appointed Dean of Women soon after the opening of the Office of Dean of Students. Because the student body was still relatively small, it was not difficult to open the office and to establish the functions under that office.

What were those functions?

We, both Mrs. Regan and I, worked very closely with student organizations on campus attempting to make these organizations more effective and to help the students have a really satisfactory educational experience. We were also closely involved in the matter of housing and in food service in the student residence halls, although both of these were directly under the business office for all business arrangements. When there were problems in the halls, we were usually involved and tried to meet with the students, with other officers in the University and to reach a solution that was satisfactory to both.

It was fairly typical during the fall semester for the students in residence halls to be well satisfied with the food service during the first few weeks. But often after the first month or two the business office would report to the food service manager that he was not staying within the food budget and that he would have to take steps to recover the losses and to keep within the budget from then on. The usual reaction of the food service manager was to overreact and cut down rather suddenly so that the students would readily notice the change. Then the protests would come from the students and we would be in the position of trying to meet with the students, with representatives of the business office, and try to reach an intermediate position that would satisfy the business office and still be reasonably satisfactory to the students.

Who provided the food service?

At that time it was not under contract, but was operated by Hensel Harrig and Bob Downie. They operated their own kitchens, hired all of the employees and in general carried all functions that were necessary in food service.

Where was it located?

Before the construction of Hughes and Beckett Halls, the food service was located in what was known as East Hall, a shingled building directly east of North and South Halls. With the construction of Hughes and Beckett, a large kitchen and food service were provided for both halls and for Titus and Struve, two halls that were added later directly west of Hughes and Beckett. In the early 1960s, the Segundo complex, four four-story halls, were built farther west along Russell Boulevard, with a central food service building to serve the four halls housing 200 each.

Much of our time was spent in counselling. Many of the problems related to academic courses, so Mrs. Regan and I spent a good deal of time talking to faculty members to identify the problem, and trying to work out a plan that would be satisfactory both to the faculty member and to the students. Very often the problem involved academic advisors putting students into courses for which they were not prepared academically. We attempted to identify such problems. For example, many students at that time had not taken sufficient mathematics in high school to prepare them for Chemistry 1A in their first semester on campus. Yet, many advisors would put these students into Chem 1A and often the results were disastrous. We found by studying the records that if students who needed to take Math D because they had not taken advanced algebra in high school would defer enrollment in Chem 1A until after they had completed Math D, they would usually perform more satisfactorily in the following chemistry classes. It was problems of this type that we tried to identify and to correct whenever possible. The fact that both Mrs. Regan and I knew most of the faculty on a first-name basis made it very easy to call them up, and to discuss the problem and very often to arrive at a satisfactory solution.

STUDENT CONDUCT, HONOR SYSTEM, WATER FIGHTS

You've already spoken about the early acceptance of the Honor System. Was the system still being honored in the '50s when you were dean of students?

As the campus grew larger, it became more difficult to carry out the complete Honor System, particularly when large numbers of students transferred in from other campuses of the University or from other colleges. Very often when students would be involved in some violations their reaction would be that they had heard about the Honor System but they didn't really believe that students would actually turn someone in for these violations. Now that they understood how it worked, they would avoid violations in the future. The Berkeley campus at one time had an Honor System, but as the campus grew larger, they found they could no longer maintain it. They dropped the Honor System and went back to a straight proctoring of examinations. Stanford was about the only other university in this area that maintained the Honor System for a long period of time. When I was in the dean of students office, we arranged through the dean of students at Stanford to have the Welfare Councils of the two campuses meet together—one time at Davis and the next time at Stanford—where they would study how the system was being operated on both campuses. I'm sure that both groups benefited from the exchange of information and from these visits.

How long was the Honor System maintained at Davis? Doesn't the spirit of it still exist?

Yes. The years when it became a real problem were from about 1958 on when the enrollment began to increase at a very rapid rate. It seemed that there were more students than could be assimilated or indoctrinated in this way of conducting campus activities. Then, there were a number of debates as to whether the system should be maintained or whether it had outlived its usefulness. I think there has always been a fairly strong thread through the campus groups that we ought to try and maintain as much of the Honor System as possible. But it has been necessary to modify it in a number of ways. Students of the 1930s and 1940s would probably not recognize the system we now have as a successor to their Honor System.

What about student conduct outside the classroom?

Disciplinary matters also came under the Office of the Dean of Students and we had a committee termed the Faculty Committee on Student Affairs. I served as chairman of this committee and other members were Mrs. Regan, as dean of women, Dr. [Harold] Reiber from chemistry, Dr. James H. Meyer, then in animal husbandry, now chancellor of the campus, and one or two others I cannot immediately recall. Our philosophy was to evaluate the student and his problem and to indicate standards that were expected on the campus. If the student was the type that we felt should be on the campus, every effort was made to solve the problem and to encourage him to continue as a student. In the few cases where it was evident that the student did not belong on the campus, either because of attitude or lack of preparation, then in case of serious disciplinary actions, we recommended suspension or dismissal to Provost Freeborn.

At the time the dean of students office was started, Dr. Freeborn, who was formerly assistant dean in the College of Agriculture at Berkeley, was appointed as provost on the Davis campus, and I talked with him extensively before I accepted the appointment as dean of students. He was very supportive and our association with the provost's or chancellor's office was very satisfactory. He used to say that on the Davis campus we really have very few disciplinary problems but when a problem occurs, "it usually is a dilly." There were a few cases where the police were involved and quite serious matters had to be decided.

Can you think of any particular cases without mentioning names?

Well, there was one case that gave us a good deal of a problem. The students in the School of Veterinary Medicine, who were mostly older students, held a party about two miles south of the campus at Tremont Hall and beer was served. On the return to the campus, one of the students stopped his car beside the road and proceeded to relieve himself. The campus policeman observed this action and followed the car into South Hall where two female students were returned to their residence. The officers then accused the driver of indecent exposure and insisted that he return with them to the site so they could prove their accusation. He did return with them and while the driver was out with one of the policemen, the other policeman shined his flashlight into the back seat where a second student, who was previously unnoticed, was sleeping. When the light was flashed in his eyes, he came out in a very belligerent way and began to attack the policeman who, at that point, pulled out his revolver and struck the student across the face, inflicting a rather serious wound just below the eye. The first thing I heard of this was early the next morning when I was called to the local jail to arrange for the release of the two students. The families of the students were very upset by the fact that one boy had been injured and both had been in jail the remainder of the night with no medical attention. The students were cited by the university police for resisting arrest and a trial was held at Dixon, because this incident had occurred in Solano County.

During the trial, the students were found "not guilty," but afterwards the parents of the students filed a claim for damages against the University. It is my understanding that eventually the case was settled out of court and that the University paid an unspecified amount as damages.

Our office was under considerable pressure from both directions. The business office, to which the police department was attached, felt that we should have taken disciplinary action against the students, and the parents, on the other hand, felt that we should be supporting the students against the police department. And so we had a rather difficult time maintaining an intermediate position. No disciplinary action was taken because the whole case was so involved and clouded that we were glad to see it settled by a dismissal of charges and by a settlement of the case.

Another less serious incident occurred a year or so later. During the period that I was dean, the students became very interested in water fights. Often this would be on the Quad or in the open spaces and they would drown each other with hoses. But then, at a certain point, water fights moved indoors and one night I was quietly at home when the chief of the University Police Department called me to say there was a huge water fight in progress on campus and that the students had entered South and North Halls. He asked me if I would see what I could do. Living only two blocks from the campus, it was very easy for me to walk over.

What did you encounter when you arrived on campus?

I found that a very large water fight was in progress in and around North Hall, South Hall and East Hall. I was also a little surprised to find that the chief of police did not come on campus and that the only two police on duty had both accompanied a student to the hospital in Woodland. So I found myself on campus alone with several hundred students armed with wastebaskets, buckets and anything that would hold water. I went from one hall to another, in general quieting things down and urging students to leave the halls. Some in North Hall had taken the fire hoses and had wet down some areas of both the first and second floor. After a matter of fifteen or twenty minutes, I was able to get most of the people to leave the halls, but as I walked between South and North Halls on the way to East, suddenly a group of three or four students ran by on their way back to their halls. One of them had a wastebasket full of water and as he saw me coming by, he let me have it. So I was pretty well drowned from top to bottom. They left the area immediately so I was unable to recognize any of the people involved.

What were the consequences of that water fight?

Afterwards, we were able to collect about twelve names of students who were actively engaged in water fights inside the halls. The business office indicated that there would be considerable damage from the water, so we called in those people that we had identified and indicated to them that there would be an assessment to cover the damages and that they would be expected to divide this among themselves. They protested that they were only a dozen out of a couple of hundred involved and they didn't feel that they should pay all of the damages. We explained to them that they were identified, that they would be expected to cover the damages, and that they could contact the other students who were involved and ask them to share in the costs. When the damages were repaired, it turned out that the damage was very nominal and only a small amount was assessed against the people involved.

I know that Provost Freeborn felt that I was a little more involved in the water fight than I should have been, but it seemed to me that the only way to get it stopped quickly was to go directly to the groups, make yourself known and ask them to leave the buildings. Thereafter we conducted quite a campaign to make the students realize that water fighting was an outdoor sport and that we would not condone water fights inside the university buildings. We offered to set up hoses and sources of water for them for outdoor water fights, but this seemed to take away the spontaneity and the fun. So gradually the students lost interest in water fights.

Did these aquatic activities occur in the springtime, or around the time of finals?

Usually early in the spring. As you know, it warms up at Davis quite early. When the juices would start running in the spring, the water fights seemed to be one of the popular sports. The women students who were on the campus at that time seemed to enjoy these fully as much as the men and were usually quite actively engaged in them.

It was the traditional way to meet someone.

Yes, a real coeducational sport. I should add that a day after the water fight, I received a condolence card, with a small verse saying in effect, "Into everyone's life a little rain must fall," etc. It was signed by a group of students from Becket Hall and they enclosed a prepaid bill for cleaning a suit at the local cleaning plant. Then, about a year and a half later, just before graduation, a student came into the dean of students office. We talked for a little while and finally he said, "Do you remember the water fight?" And I said, "Yes, I always will," and he said, "Well, do you remember that somebody threw water on you." And I said, "Yes." And he said, "Well, I did it! And I didn't want to graduate and leave here without telling you that I was the one who threw the water."

Most of our contacts with students were good-natured and most of them were settled without any serious consequences, so the experience as dean of students was first of all very interesting. I felt at that time that I knew personally probably half the students on campus and had an opportunity to either meet with them in group meetings or individually. And in many cases I'd been able to help them, or in some way make their college experience more meaningful.

CAMPUS EVENTS AND TRADITIONS

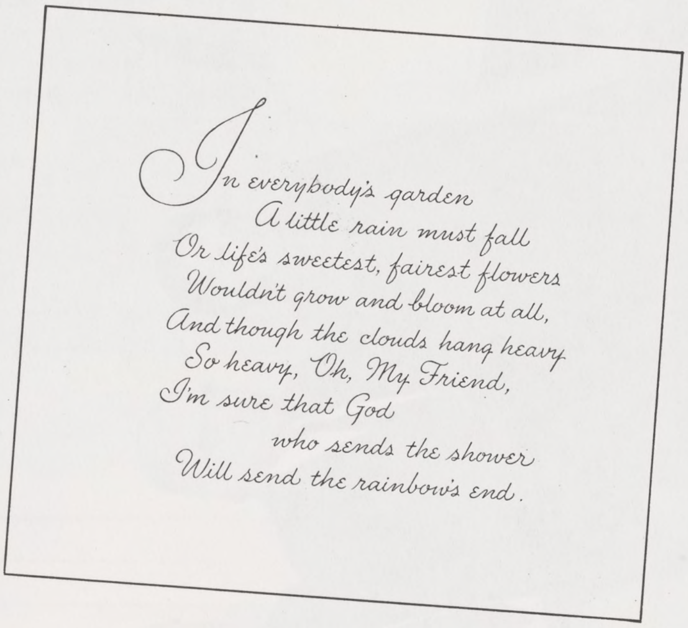
Picnic Day traditionally involves both students and faculty. Can you recall other campus events that involved the cooperation of both?

Well, each year they held a Judging Day where groups of students were brought in from high schools all over northern California. They first had the livestock judging and this was expanded to include the judging in fruits, agronomic crops and soils, I believe. I was not directly involved with this, but we would hear of the Judging Day and see the numbers of students that would come. Then each summer the 4-H clubs would come to campus where they were housed in tents. They held meetings and had various activities for several days while they visited the campus. Now they stay in the residence halls during the summer recess, so students aren't so involved.

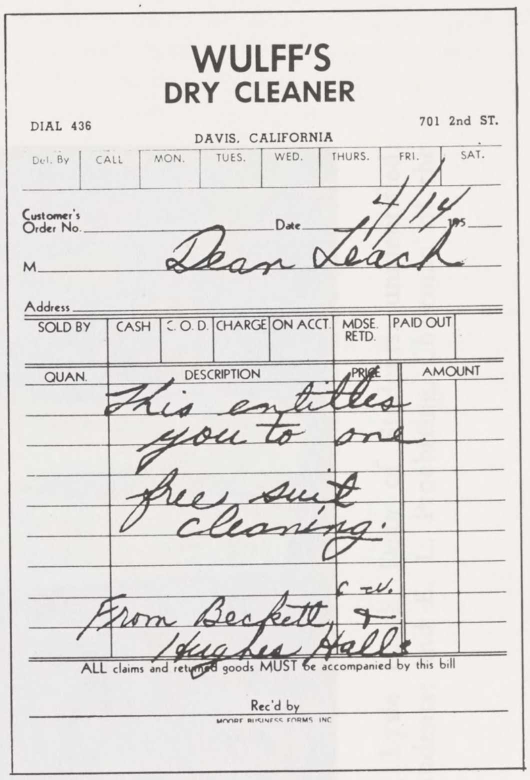
Do you have any recollections of the Leap Year Labor Day activities?

Yes, I have some very vivid recollections there because I was quite involved in a few of them. When we came to campus in 1929, we heard that they had a program here where each Leap Year, on the 29th of February, all classes were dismissed and everyone—students and faculty—turned out to do something that would be useful to the campus as a whole. The first Labor Day after we came was in 1932. That year they decided to put the sprinkler system into the Quadrangle. We dug ditches by hand and laid two-inch pipe for the sprinkler system.

That had previously been a barley field, hadn't it?



In everybody's garden
A little rain must fall
Or life's sweetest, fairest flowers
Wouldn't grow and bloom at all,
And though the clouds hang heavy
So heavy, Oh, My Friend,
I'm sure that God
who sends the shower
Will send the rainbow's end.



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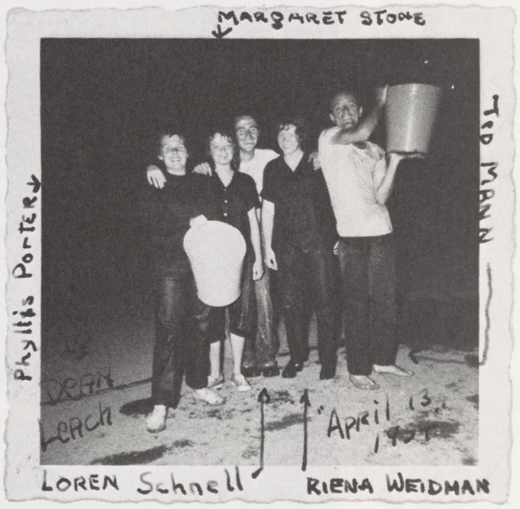
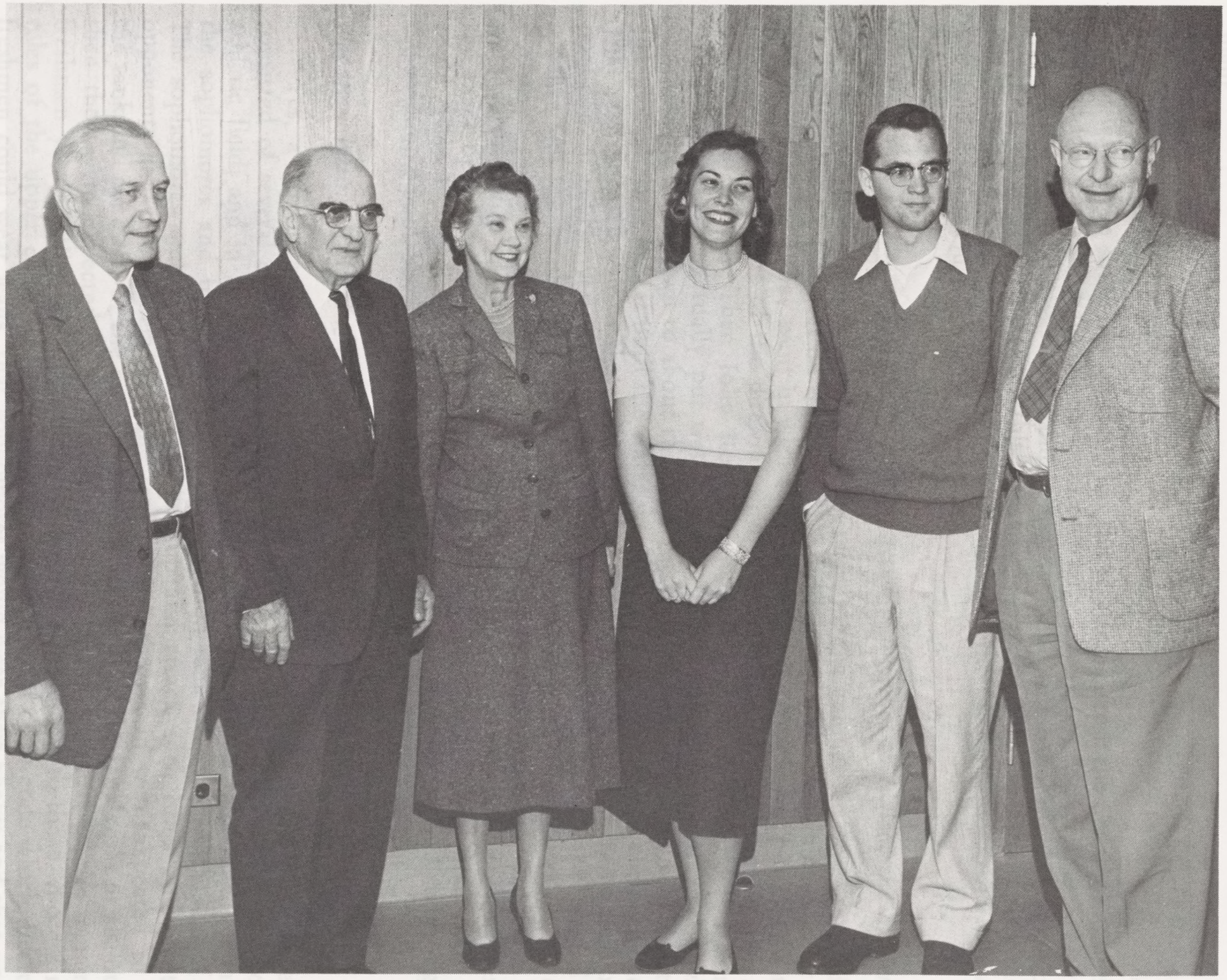


The great student water fight of April 13, 1954.



Presentation of student scholarships. *Left to right:* Lysle Leach, Dean of Students; unidentified; Susan Regan, Dean of Women; two unidentified students; and E. L. Proebsting, Chairman of the Faculty Scholarship Committee (c. 1954).

Yes, that had been a barley field and then they grew alfalfa there, by using flood irrigation. After the sprinkler system went in, they could plant trees and start to develop a lawn in the Quadrangle. I think that was the same year that the sundial disappeared one night in a huge blast. Someone had planted dynamite under this sundial and blew it up in the middle of the night.

Was it in the Quad?

It was at the south end of the Quad, directly across from the Classroom Building. Someone had the idea for a sundial—and had poured a large chunk of concrete there which was to be the base of the sundial. Others either didn't approve of the sundial or didn't approve of its location. So unknown individuals planted this rather large bundle of dynamite and blew the whole thing into chunks of concrete, and it was all hauled away the next morning. And that was the end of the sundial in the Quad.

Another year didn't they start to develop a swimming pool to replace the old irrigation tank?

Yes, in 1936 when they were making plans for the gymnasium, someone had the idea that it would be a good project on the 29th of February to dig the swimming pool. It was surveyed and staked out and they went in with some earth-moving equipment, but mainly it was done by hand. They took part of the dirt out with horses and scrapers and there were a lot of people digging with shovels. Then John Rogers thought it would be a wonderful thing to build a swimming pool and actually put water into it the same day. In the afternoon when they had removed the earth down to about the ultimate depth of the swimming pool, he started a pump and poured water into it until the water was four or five feet deep at the deep end of the pool. That was an invitation to the students. They immediately began to throw each other into the pool and, of course, very soon it was just a matter of thin mud. Then they started throwing the coaches and the faculty in physical education into the pool. From there on, no one was safe. Several of the faculty members—I remember one in veterinary medicine and one in engineering—were thrown into the pool. I saw this coming, so I left, going in a northwesterly direction, the exact opposite of where I lived. But the point was to get away from there as quick as I could. Then I circled around the campus to make my way home. So I did not get thrown in the swimming pool, but it was a close call.

Was there hazing of freshmen?

Yes, a certain amount. Each year they would have a Freshman-Sophomore Brawl. This consisted of several events. There would be a tug-o-war and usually a push ball contest on the athletic field, and in the early thirties, this would close with a tank rush, a contest between the sophomores and freshmen at a small irrigation tank near the south side of the campus. The sophomores and freshmen would line up and attack each other and try to drag their opponents and throw them into the tank. Those thrown in were expected to swim across the tank and were out of the contest. If the freshmen lost the tank rush, or the other contests, then they were expected to wear their freshmen dinks for most of the remainder of their freshmen year. A Vigilante Committee meted out punishment to those who did not obey the rules of the upper classmen. There was also a fairly strict dress code which dictated that only freshmen could wear jeans, that sophomores would wear their khakis and that upper classmen were the only ones permitted to wear cords. Sometimes this campus dress was worn without washing for long periods of time, so the common idea was that cords or jeans would stand in the corner by themselves.

What about the "Hi Aggie," or the "Cal Aggie Spirit." Is it still alive and well? It seems to have been a trademark of the campus.

I'm afraid it is not very well or very commonly observed today. In the first years as you walked across campus, almost everyone you would meet would say "hello" or "hi there" or would speak to you. It was a very friendly campus. The visitors were very surprised to be greeted by students and faculty as they walked across the campus. This lasted until about World War II, but after that when the numbers were much greater, this "hello" spirit or "hi" spirit seemed to disappear. Now you can walk across the campus and very few people outside of your own acquaintances will recognize you.

RETIRING DEAN "STEPS DOWN TO RESEARCH"

You returned to being a full-time plant pathologist in 1958. Why did you leave the dean of students position?

At that time, Chancellor Freeborn was approaching retirement age and they were considering the appointment of his successor. I had had very good relations with Chancellor Freeborn and I felt that when a new chancellor came in it would be preferable for someone else to hold the position of dean of students. Also by that time, I had reached the age where I had just about ten years to go before retirement and I felt I had taken too much time out of my teaching and research, and so I asked to be relieved of the Office of the Dean of Students.

Previously, when I had been on a sabbatical in 1956, they had asked Professor Bill [William C.] Weir from animal husbandry to take the position on a six month appointment. He served very effectively as dean of students and appeared to enjoy the responsibility and the activities. So it was decided to appoint Bill to that position and he served as dean of students for eight years from 1958 to 1966.

Is it true that when you relinquished the dean's title, the student newspaper announced that you were "stepping down to research"?

That was approximately the idea. Most of my friends who were in research thought that was a little different evaluation than they would put on the various activities.

WEIR NEW UCD DEAN

Dr. Leach Steps Down To Return To Research

By Sue Kugler

Dr. William C. Weir will become dean of students on the UCD campus at Davis next July, Provost Stanley B. Freeborn announced.

Weir, associate professor of animal husbandry, will succeed Dr. Lysle D. Leach, dean of students for the past six years, who has asked to return to full-time teaching and research as professor of plant pathology. The provost said Dean Leach found his duties as dean cut too heavily into research projects which he desires to complete. Dr. Leach commented, "I have certainly enjoyed working with the students and would like to express my appreciation of the 'cooperation'." He indicated that his participation in student activities will continue with such offices as Faculty Athletic Advisor.

The newly appointed Dean Weir has worked closely with students since his arrival on the Davis campus in 1948 from Oregon State College. He has served as a departmental counselor, as chairman of the Committee on Undergraduate Scholarships and Awards, and as adviser to Blue Key honor society, in addition to a period as acting dean in the spring of '56 while Dean Leach was on sabbatical leave. While serving half-time as Dean of Students, Weir will continue his teaching and research in the Department of Animal Husbandry and will carry on his sheep research at Davis and University's Hopland Field Station.

Dean Regan commented that she was "exceedingly sorry to see Dean Leach leave" but feels that since both men are "great friends to the students" we can look forward to a very successful new administration.

The feeling of many students is reflected by Lee Adler, ASUCD vice president, who commented, "I'm sorry to see Dean Leach go. There is no one word which can express the job that he has done. He has looked out for student welfare and his cooperation and receptiveness has been outstanding."

From *The California Aggie*, May 8, 1958.

IX

GROWTH AND EXPANSION AT UC DAVIS

I thought maybe you might comment on some of the major administrative and academic changes that have affected the growth and development of the Davis campus. You've talked about some of the early administrators, but the biggest change came in 1952 when there was a reorganization and the Davis campus was given some autonomy with its own provost who was elevated to a chancellor in 1958.

Yes, I think this change was occasioned by the establishment of the College of Letters and Science in 1951. Previously the campus had been oriented to agriculture, later adding home economics [in 1936] and the beginnings of the School of Veterinary Medicine [in 1947].

Were there problems getting California's only veterinary school located at Davis?

The livestock industry was very anxious to have the School of Veterinary Medicine established in California and was instrumental in securing legislation and funding for this project. At that time Washington State College and Colorado State College were the only schools in the western United States with veterinary schools. Since the Davis campus was the location of the animal husbandry department, it was assumed that veterinary medicine would also be here.

Some of the administrators at Berkeley decided, however, that the new school should be located there because of the close association between animal health and human, or public health. When the leaders of the livestock industry learned of this development, they exerted strong political pressure on the University and a final decision was made to locate the School of Veterinary Medicine at Davis.

Concerning letters and science, departments such as chemistry, physics, English, history, etc., that had formerly been teaching departments within the College of Agriculture, were then established as separate departments within the College of Letters and Science. It was anticipated that there would be a very rapid growth of this college on the Davis campus. So they planned what was essentially a reorganization of the administrative portions. A provost was appointed on the Davis campus in 1952. A dean of agriculture—at that time Dean [Fred N.] Briggs—was appointed on the Davis campus, with a separate dean of agriculture on the Berkeley campus, both under the vice president, agricultural sciences, Harry R. Wellman, who succeeded Claude B. Hutchison [in 1952].

It turned out that the College of Letters and Science during the first few years grew much slower than had been anticipated. But a few years later when the campus became better known for instruction in the letters and science field, the enrollment did increase very rapidly and it's fortunate that they had a few years to prepare for the numbers that came later.

That meant bringing in a great many new faculty members in the fields that hadn't been represented at Davis before, didn't it?

Yes. Up to that point, the faculty in agriculture far outnumbered the faculty in all the other fields, but this didn't last long, because large numbers were brought in to the sciences and humanities in the College of Letters and Science. Many of them were transferred from other campuses of the University of California, but large numbers were also employed from other universities.

Was there general acceptance for this among the faculty, or were some concerned that agriculture would lose its former predominant position?

There were mixed feelings on this. Some who had felt very strongly about the independence and predominance of the College of Agriculture felt that this was going to change the whole character of the campus and not in the direction that they would like to see. But others felt that balancing the agricultural, biological and physical sciences by the development in the humanities and social sciences would have a very favorable effect on the future of the campus. There was not total agreement, and even as it developed, there were many people who deplored the way it was changing, whereas others felt that these changes were essential for a well-rounded university.

How did you feel about it?

I was in favor of the expansion, even recognizing some of the things that we had maintained as a predominantly agricultural campus were being lost. I think the benefits far outweighed any disadvantages that occurred.

Another major change occurred in 1959 when the Regents decided that Davis would be a general campus of the University. That brought in a whole new influx of different disciplines. Chancellor Mrak was in charge during those years. Do you recall any of the special problems that arose at that time?

The first thing leading up to this was the change in the names of campus administrators. Freeborn was the provost up to about 1958; then he was named chancellor. The so-called provosts on all the smaller UC campuses became chancellors. This, I think, was an indication of what was to come. When Freeborn retired in 1959, Emil [M.] Mrak, who was professor of food science and technology, was appointed as the new chancellor. This, as you said, was the time when Davis became a general campus of the University. A full College of Engineering was established on the Davis campus [in 1962], whereas previously we had only a Department of Agricultural Engineering. The College of Agriculture changed its name to the College of Agricultural and Environmental Science to indicate its increasing concern with land, air and water resources and in consumer sciences. Other additions were the School of Law, School of Medicine and most recently the Graduate School of Administration.

PLANNING AND BUDGET COMMITTEES

Were you involved in planning facilities to house expanding departments and administrative staffs?

I was on a few of the planning committees for individual buildings. At one time, in 1950, I was on the committee planning the administration building, at a time when it was planned to be built directly west of A Street. The area where Voorhies Hall now stands and the open space to the north was supposedly the site for this proposed administration building.

Why wasn't it built there?

Let me explain. From about 1930 to 1952 Claude B. Hutchison, who held the title of dean of the College of Agriculture and was housed on the Berkeley campus, was the man who administered the Davis campus for the University. He had a great deal to do with the building of the Davis campus and of the strengthening of a number of departments, including plant pathology. He was made the first vice president of Agricultural Sciences in about 1951 and had very strong opinions on the best way to administer the College of Agriculture and the experiment stations statewide. In informal conversations, he often expressed the view that his idea of an ideal arrangement for the administration of agriculture in California would be to have two strong centers for agricultural instruction and research, one at Davis and one at Riverside, with ample communication by phone, car, or airplane. On the other hand, President Sproul seemed to favor the location of all statewide administration at Berkeley.

This difference surfaced during the planning of an administration building for the Davis campus. In about 1950 a planning committee was appointed with Professor Harold Reiber as chairman and with me as one of the members. As I've said, the administration building at that time was planned to go on the open area west of A Street and between First and Second Streets. At early meetings, we began to compile a list of various functions that would be housed in this building. Dean Hutchison indicated that it was probable that his office would be located in this building and that the statewide office of agricultural extension would probably be moved to Davis [from Berkeley] and housed in, or adjacent to the new administration building on the Davis campus, and that we should provide suitable space in this new facility for these statewide offices. These planning sessions were always attended by representatives of the Regents' architects at Berkeley under Mr. [R. J.] Evans. I understand that after one of these planning meetings, the architects returned to Berkeley and reported to President Sproul: "It appears to us that the administration building committee at Davis is actually planning the future statewide administration in the College of Agriculture. Do you want them to do this?" Sproul replied: "Certainly not!"

It developed that this building planning committee was never called for another meeting. When I returned from a Marshall Plan assignment in Denmark and Ireland at the end of 1951, I found that Vice President Hutchison had resigned from the University and had accepted a position first with the ECA in Pakistan and later at the University of Nevada. The administration building that we were supposed to plan was never built, and later, another committee was appointed to plan the building now called Mrak Hall [constructed in 1966].

Did you ever serve on the campus budget committee?

I was appointed to the Committee on Budgets and Interdepartmental Relations in 1950 and served for a little over a year. Then I went on a six month leave and soon after my return I accepted the position of dean of students and asked to be relieved from my appointment on the budget and interdepartmental relations because that consumed a great deal of time and was one of the most active committees on campus.

That committee met weekly, didn't it?

Yes, it did.

ACADEMIC SENATE, THE LOYALTY OATH

From the point of view of the plant pathology department, how involved were faculty members at Davis in decisions of the Academic Senate when it was still based in Berkeley?

During that period to vote in the northern section of the Academic Senate, it was necessary for faculty members to drive to Berkeley, usually spending an afternoon and part of an evening in order to be able to participate in the discussions or to vote on issues. During the time when the loyalty oath was under consideration [1949–1951], there was a very active discussion at Berkeley and numbers of faculty would go down each week and spend quite a few hours trying to maintain their position. This took so much out of your regular work time that there was a strong movement to localize the Academic Senate. They next decided the Academic Senate should have a separate group on each campus.

How did you feel about the loyalty oath? Was it a problem for you?

Well, it was an unfortunate situation. It was brought out with the idea that this action of requiring a loyalty oath for all the faculty would allay the fears of people in the state who felt that the University was too liberal and that perhaps some of its faculty members were involved in socialistic or communistic activities. The Regents were somewhat reluctant to pass such a requirement, but President Sproul felt that the University would benefit from the statewide support as a result of this action. He didn't realize how much adverse response would occur within the faculty itself. The discussion in the Academic Senate related to whether the faculty would support this requirement. In fact, there were a number of individuals in the faculty who refused to sign the oath. Some of them left the faculty for a period of time and really very little positive was accomplished as a result of it. It was a very unfortunate period in diffusing the support of the University from its faculty.

Were you able to participate more in the Academic Senate once there was a section established at Davis in 1963?

Yes, it gave a much better opportunity for individuals to participate in the discussions and to better understand the problems that were coming up. The difficulty was that you had action then by individual Academic Senates on the different campuses. The problem was to bring unity statewide. So a special reorganization committee was appointed and I was on that committee. There were one or two faculty members from each of the university campuses. Trimble Hedges and I served on that committee. We met regularly over quite a period of time. What grew out of that recommendation was the Representative Assembly; one for the north, usually meeting at Berkeley, and one for the south. This then meant that it was not necessary for all the faculty members to attend the meeting to have their vote recorded. Representatives which they elected would go to the Representative Assembly and vote for the larger group which was not required to attend those sessions of the Academic Senate.

CAMPUS UNREST IN THE '60S AND '70S

From your vantage point as a former dean of students, how did you view the student unrest that occurred on the campuses of the University in the '60s and '70s, particularly here at Davis?

Well, my first reaction was that I was extremely fortunate not to have been in an administrative position at that time because it was very difficult to carry out administrative activities within the University. This was a very unfortunate time in the life of the University itself. It seemed in the beginning to be developed by a relatively small but very active group. It broke out first at Berkeley, but from word that we received from national sources it was indicated that the national student organization had met at Antioch College in Ohio the previous summer [in 1964] and they had discussed starting what amounted to a rebellion on some well-known campus. The decision was whether the first effort should be at the University of Wisconsin or the University of California at Berkeley. The Berkeley campus was selected. This group came in the following fall to the Berkeley campus [The sit-in demonstrations at Sproul Hall began in October 1964].

You mentioned a national students' organization?

I don't know what organization was involved but they were very well organized. They seemed to have been remarkably effective in dividing the campus and pitting one part of the campus against another. The faculty and the administration began to work at cross purposes. Some groups of students were very antagonistic to this rebellious attitude, and wanted to use force to stop the demonstrations. President [Clark] Kerr, who was out of the country at the time this first developed, returned and found the campus in disarray. He tried to correct some of the things that had happened and was not very effective because the students made agreements with him and then went out and announced publicly agreements which were not in accordance with the decisions that had been made in the first meetings. Later [in 1966], Ronald Reagan was campaigning for governor and he found that the most popular issue in the state was to cure "the mess at Berkeley." He ran very strongly on this point which won him, apparently, many votes and he was elected. Most of his supporters thought that the answer was to fire the president of the University. When Clark Kerr asked to have his situation clarified, this made it very easy for the Regents to release him from his administrative activities and his tenure as president of the University was terminated [in January 1967].

How was that news received at Davis?

I think most of the people at Davis were disappointed that President Kerr had been forced out. They felt that Kerr was a good administrator and was doing the best he could to try to bring order out of the chaos that occurred. But under the conditions I'm not sure that anyone could have really been successful. On the Davis campus we had a certain backwash from the activities at Berkeley. But usually the activities at Davis would come later than those at Berkeley; they would involve a smaller percentage of the student body, and they were much less violent. The Davis campus was not damaged by the student unrest nearly as much as the Berkeley campus itself. I think the fact that you had on the Davis campus a large group of fairly conservative students in agriculture and engineering maintained a balance wheel that did not occur at Berkeley, where these groups represented a much smaller percentage of the total.

All this occurred against the backdrop of the unpopular Vietnam War, which was magnifying problems and causing increased activitism, particularly among students. Did you ever foresee that students would also take an active interest in local government as they have done in the Davis community during recent years?

No, I did not foresee this fundamental change in local government. The numbers of students were relatively small until the 1960s and most of the undergraduate students were under twenty-one years of age and, therefore, ineligible to vote. They seemed content to use their energies in campus politics and associated student activities. When the voting age was lowered to eighteen years [in 1972], nearly all college students were eligible. The state Supreme Court ruled that college students could declare their residence either at the home of their parents or at their college residence.

Politically active students saw an opportunity to register large numbers of students in college towns and to exert a strong influence on local elections, especially where large universities are located in relatively small towns such as Davis and Chico.

Lysle, unless you have more you'd like to add, next it would probably be best for you to discuss the scientific aspects of your many research projects in a series of interviews with your colleague Jack Hills, an extension specialist with whom you have worked for a number of years. Later, perhaps you and I can conclude with more discussion about your family and your post-retirement activities.

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EARLY RESEARCH AT THE UNIVERSITY FARM

The interviewer is Jack Hills.

Lyle, first of all it would be informative to know why you came to Davis and what sort of work you started out on when you arrived.

Earlier I described my move from Iowa State to California and the fact that I reported for duty in Berkeley and was located at the UC Deciduous Fruit Station at San Jose from April through July of 1928. My job at that time was to work on strawberry diseases and this included investigation of some root diseases affecting strawberries in the Watsonville area and the Santa Clara Valley.

Had you completed your Ph.D. at this time?

No. When I accepted the offer to come to the University of California it was with the understanding that I would be permitted to select a research problem that could be used as a thesis for a Ph.D. degree at Iowa State College. In addition to the root rot problems, the job involved selection among strawberries for resistance to a serious virus disease called xanthosis, or yellows. However, it soon developed that there did not seem to be any really good research problem that would be suitable for an acceptable thesis and I was somewhat concerned as to whether I would be able to fulfill that part of my obligation. With this in mind, Professor Smith transferred me to Berkeley at the end of July 1928.

He was the chairman of the Department of Plant Pathology at that time.

That's right. He was the chairman over Berkeley, Davis, and several substations. This move was to give me a better chance to work on a research problem. After a few months, Professor Kendrick who was the vice-chairman, and located at Davis, suggested to Professor Smith that it might work out better if I were transferred to Davis because there were a number of research problems there that probably would be more suitable for a thesis problem. So on the first of February in 1929, Jane and I moved to Davis.

GARDEN BEET SEED, 1929

One of the first problems that appeared was a very serious disease on garden beets that were grown for seed in the Sacramento Valley.

Whereabouts in the Sacramento Valley did they grow most of this garden beet crop?

A great deal of it was grown in the Sacramento Valley near Clarksburg and on both sides of the Sacramento River within fifteen or twenty miles of Sacramento, in the northern part of the Delta area. The disease was downy mildew and when I first saw the seed fields, a very high percentage of plants showed distortion, curling of the heart leaves and an abundant sporulation of the downy mildew fungus. So there was no question about the cause. The infected plants developed seed stalks in March and April and the fungus, which was internal in the crown and heart leaves, grew systemically into the young seed stalks. The seed stalks themselves were very poorly developed and a number of secondary stalks formed on the crown. When the plants began to flower and to produce seed, many of the seed stalks would dry up and produce little or no seed.

Were there any sugar beets grown around Clarksburg at that time, and did you see any effect on the sugar beets at that time?

Yes, there were but they were all spring planted. At first I didn't see any downy mildew on the sugar beets, but later in the spring the same disease appeared on the younger leaves of a great many of the sugar beets.

These garden beets then would have been planted the previous fall?

Yes. Seeds of garden beets were planted in root beds during late summer or early fall. During the fall months, the beets grew and produced a root that might be from a half inch to an inch in diameter. During the winter, these roots were lifted and transplanted to seed fields where they were spaced approximately three feet in each direction.

So the first thing, after Dr. Kendrick suggested that I investigate this problem, was to find out how the disease developed and what could be done to control it. I followed the development of the disease in the field and collected plants that were systemically infected with flowers in different stages of development. These were preserved in fixing solutions and embedded in paraffin blocks for sectioning and staining for microscopic observations. The first thing I tried to do was to determine the relationship between the fungus and the various tissues of the plant, and particularly the relation to the flowering parts and to seed development. I also undertook to determine the cardinal temperatures for spore germination, for infection of young plants and for the sporulation of the fungus.

When we talk about cardinal temperatures we mean what kinds of things?

By this I mean the minimum, optimum, and maximum temperatures at which spores would germinate and infect seedlings and also the conditions under which the fungus would produce spores and spread to other plants. We had very limited equipment at that time for control of temperatures, but I was very fortunate that the truck crops department, now called vegetable crops, had a field headquarters with a number of walk-in chambers maintained at different temperatures. The truck crops staff was very generous in letting me use parts of these chambers and this is where I prepared my spore mounts, used a microscope to determine the rate of germination and measured the effect of temperature on the infection of seedlings. From the microscopic studies, it was clear that the fungus, which first showed up as localized spots on leaves of the beet, rather quickly attacked the young heart leaves and that the mycelium grew down into the crown. Thereafter, all newly-formed leaves were systemically infected.

Did you just prepare a suspension of spores and spray it on the seedlings to make these observations?

Well, some of the observations were made in the field, but they were also repeated under controlled conditions by spraying a suspension of spores on young seedlings in moisture chambers.

This particular fungus is very difficult to work with in artificial media, is that it?

Yes, it is known as an obligate parasite, that is, it cannot be grown in artificial media. Therefore, it was necessary to work with living plants and to maintain cultures of the fungus on the growing plants.

One of the things I found from free-hand sections and prepared slides was that the fungus grew directly into the flower parts and the developing seeds and there was internal mycelium within the seed tissue in very close association with the ovule that would develop into the seed. I also found that the fungus was producing oospores abundantly in the leaf tissue and in the tissue of the flowers.

What kind of spores are these oospores?

Oospores are the resting spores that develop within the plant tissue. They have a very heavy wall and persist for a long period of time and have the possibility of surviving in the soil and initiating infection during subsequent seasons. The spores which cause the infection in the field or in the root beds are very thin-walled spores that are produced on tree-like branches protruding from the breathing pores or stomata of the leaf. These spores are produced very quickly from internal mycelium and can be wind-blown or drift to other plants and there, on the surface of the leaf, the spore germinates to produce a tube-like structure that penetrates through a stoma to produce mycelium between the plant cells. Food for the fungus is obtained by branches that penetrate the thin-walled cells. After a few days of growth, tree-like structures grow through breathing pores on both sides of the leaf and produce large numbers of egg-shaped spores that are wind-blown to other plants, and thus spread the disease.

I also wanted to study the host range so I wrote to the Vilmorin Company in Paris, France, for collections of the seed of many closely related plants and weeds. I found that this downy mildew would produce infection only on closely related members of the Beta genus such as garden beet, sugar beet, swiss chard, and other species of the genus Beta. It was not possible to produce infection on spinach or closely related weeds such as lamb's quarter, which belongs to the same family but not to the same genus as the garden beet.

So all of this laboratory work and careful observations in the field were necessary to learn how this fungus developed, its relation to the host, how it could be transmitted to other crops, and to what other crops it could be transmitted. Then this led you where, into what next?

Well, the following summer the root beds were planted near Sacramento, and during the fall months I observed that the downy mildew appeared as isolated lesions on the leaves in the root beds, spread from plant to plant, and as it went into the winter months, the infection developed in the crown leaves and became systemic in the crown. When the roots were lifted during the winter, and transferred to fields, the infected plants were carried along and these were the plants that produced little or no seed. It seemed very important to protect the plants in the root beds to avoid initiation and spread of infection and at the time of transplanting to eliminate all plants that had systemic infection in the crown. It was very clear that the systemically infected plants would produce little or no seed and, therefore, it was important to eliminate them, both because they would not produce a satisfactory seed crop and because they would serve as the source of inoculum that would spread to additional plants in the seed fields.

Control measures such as spraying with copper compounds were tried and seemed to have some effect, but once the fungus had developed in the crown, there was little or no chance of controlling it by the fungicidal sprays.

Also, I suppose at that time, we didn't have very much in the way of fungicides that were systemic and could get inside of a plant and kill fungi there.

There were no fungicides at that time that were known to be systemic. I'm sure that if some of the fungicides that are now available were known at that time, we would have used systemic sprays which might have controlled the problem directly in the seed bed.

The other thing that was noticed by observations in many different root beds and seed fields is that some of the commercial varieties were much more resistant than others. In cooperation with the seed companies, such as Ferry-Morse, Pieters-Wheeler and Rhonert, some observations were made on the relative susceptibility of different varieties. The seed companies then were encouraged to make further selections with the idea of developing resistance. Over the years that followed, several of the companies developed strains of the more important varieties of garden beets that were much more resistant than the ones that were in common use at the time I studied this disease.

One point that you might just say a little bit more about is how important was the infection of garden beet seeds in transmitting this disease to the next crop.

Our studies of prepared slides showed a very intimate association between the mycelium and the young seeds and that oospores were frequently produced in the corky sepals of the beet seed ball. This suggested the possibility that this fungus survived within the seed parts and could be carried into another year's planting, either internally in the seed itself, or as oospores in the corky tissues that make up part of the beet seed ball. With that in mind, I planted large numbers of seed collected from infected plants and made an attempt to determine whether infected seedlings would develop from them. Among the seeds collected from infected plants, a very small number showed infection of downy mildew on seedlings under conditions where they were isolated and protected from outside contamination.

In other words, that indicated that the infection was really coming from the seed infection.

This was the indication. The fact that you could never be positive that no external contamination might have been introduced during observations or watering made it impossible to be certain that this represented seed transmission. But the evidence would indicate a very strong possibility of seed transmission of this downy mildew.

Also, I collected plant tissue, principally leaves containing large numbers of oospores, and this material was fragmented and mixed with soil. Seeds from disease-free sources were planted in this soil in isolated growth chambers. In one case, a few infected seedlings appeared, but in other trials, no infection resulted from the addition of the oospores. Here again, the evidence is not conclusive that oospores were serving as the source of infection, but there was a strong possibility that infection in new areas could result from infected seed.

To get an epidemic of a disease of this type, then, you would probably have to have a number of plants producing these asexual, wind-blown spores, but the oospore stage could maintain this organism in nature for indefinite periods of time, I guess.

Yes, that was our conclusion. In a root bed, one that I followed very closely, the infection first appeared on a single plant and over a period of six weeks, it spread from this plant to cover more than half of the entire root bed and, at that time, infection appeared at another location some distance from the first one and also spread from that area so that nearly the entire root bed was involved in the infection. So apparently it required only one or two initial infections to lead to involvement of the entire root bed.

The question still remained as to whether seed transmission could be proved. I had described these first experiments in a publication.

Was this published in Hilgardia?

Yes, my first publication [in California] was in *Hilgardia* in 1931. A few years later, a plant pathologist in Australia found a field of garden beets that was infected by downy mildew and this was the first time this disease had ever been observed in Australia. So he undertook an examination of the seeds used for this planting and found, as I had described, numerous oospores in the corky sepals of the beet seed balls. He concluded that since this was apparently the first case of downy mildew in garden beets in Australia, there was a strong probability that it was carried in with the seed. I traced back and found that this seed was produced in California, in an area where downy mildew was quite severe. So I think circumstantial evidence would indicate that this fungus was introduced into Australia with seed grown in California.

XI

DOWNY MILDEW: SOLUTION IN SEED SELECTION

I mentioned earlier that the same disease did appear on sugar beets in some of the fields in the Sacramento Valley. And when I had pretty well completed my trials with the downy mildew on the garden beets grown for seed, I made a survey to determine how common the occurrence of downy mildew was on sugar beets.

This would have been about when, Lysle?

This was started in 1930. I found that while the disease was usually not very severe in the Sacramento Valley, it was a very common and a very damaging disease in the coastal valleys such as Salinas and Santa Maria. In some years there also was moderate infection in some fields in the lower Sacramento Valley.

Travel from Davis to Santa Maria in those days wouldn't be nearly as simple as now, I guess.

Well, travel was primarily with a university-owned Model T Ford, so it was a little slower. The roads were not as good, but we had enough transportation with Model T's that we could get around the state quite well.

But to go to Santa Maria and back, that would be probably a pretty good week's outing, wouldn't it.

Well, it certainly would involve several days to leave any time for field observations. I found that in an area like Salinas or Santa Maria, in some years, we had as high as sixty or eighty percent of the sugar beets infected by downy mildew.

During this part of this survey that you made, did the sugar companies seem to be quite aware of this disease or hadn't they really given it much thought?

They were aware of it because in the reports of an experiment station supported by Spreckels Sugar Company at Salinas, early reports indicated that in some years downy mildew was quite severe. But apparently no one had made any attempt to determine the effect on productivity of the sugar beets, nor had anything been done for control.

SUGAR BEET EXPERIMENTS AT SPRECKELS, CA., 1930

So in 1930 I had my first experimental plot in sugar beets near the Spreckels headquarters that's near Salinas.

Do you remember some of the people that were involved in the management of Spreckels in Salinas at that time?

I remember that Mr. [Charles L.] Pioda was the general manager at Spreckels and when he retired, George Wright, who was at Sacramento, was transferred to Salinas. Both of these gentlemen were very cooperative in arranging experimental plots for me and in securing the cooperation of growers and field men.

One of the first things that I attempted to do was to get some measure of the effect of downy mildew on the yield of sugar beets. By comparing the size and sugar content of healthy and infected beets, it appeared that the yield of roots was reduced by twenty-two percent and the available sugar by thirty-nine percent as the result of infection by downy mildew.

These were the results of your program to assess the effect of downy mildew on sugar beet production in the Salinas area.

FIELD TRIALS AND SEED SELECTION, 1930–1939

The first field trial was in 1930, but later, in order to measure the effect of the disease on the productivity of sugar beets, we made several field plantings: one at Davis in 1935, one in Santa Maria in 1937 and one at Salinas in 1938. At Salinas we established a good sized field plot and as the downy mildew appeared on the young plants early in the spring, we took stakes that were painted with a specific color and placed them beside the infected plants so that the very early infected beets would have a stake of one color and the beets infected a few weeks later would be staked with another color and on through the season. In this way, we could identify each beet in the crop according to the time that infection first appeared.

At harvest time it was clear that beet plants infected very early in the season were severely dwarfed in size and if infected within 100 days of emergence, the roots would average less than half of the uninfected plants. Two graduate students at Davis, Byron Houston and Jack Oswald, were assisting me with this field work.

So you were just comparing infected and uninfected plants within this plot. You'd dig up large numbers of roots of each type?

Yes, we'd measure their weight and sugar percentage, so we could calculate sugar production according to the time at which infection had occurred. These trials showed that sugar beets that became infected after they were more than 100 days old were not severely damaged, and that plants infected after they were 150 days old showed practically the same yield as uninfected plants.

By June when the weather was becoming dry and somewhat warm, the infection would stop and many of the plants would recover, so that late in the summer there was very little evidence of the downy mildew infection. It was clear that the sugar production was very much lower on infected plants than on noninfected and the most severe damage occurred when the plants were infected when they were still fairly young.

These plants that were infected early, Lysle, apparently their foliage just didn't grow, leaves just didn't develop.

On sugar beets there is very little localized infection on outer leaves. It is the very young leaves in the heart of the plant that become infected. The mycelium grows directly into the crown and subsequent leaves are systemically infected, so the plants were dwarfed and did not show much recovery until after the period of downy mildew activity had passed.

By observing different varieties, we noticed a considerable difference in susceptibility. To measure this, we made plantings of selected varieties grown from European seed and from the newly developed curly top resistant varieties for which the seed was increased in the southwestern part of the United States. Some of these varieties appeared to be quite resistant, but many of the varieties, including some of the new curly top resistant varieties, were very susceptible to downy mildew. This suggested that perhaps the most important method of control would be the development of a high level of resistance among the preferred sugar beet varieties. Plantings were made both at Santa Maria and Salinas to get some information on this topic.

This would have been about when?

This was 1937 and 1938. It would be possible to select uninfected beets under field conditions and increase the seed from these plants. However, the chance of their being escapes rather than resistant plants seemed very great, because usually the infection in the field would amount to only forty, fifty, or sixty percent. So the infection was really not severe enough to make a clear separation between susceptible and resistant plants. The fact that most of the infection occurred on the very young leaves suggested that young seedlings could be inoculated to secure a much higher percentage of infection; and that the progeny of such selections could then be retested in the seedling stage to determine if improvement in the resistance of selection had occurred.

In other words, a technique like this could greatly speed up and improve a selection program for developing resistance. You don't have to go to mature beets and grow them out in the field; you can do it almost all in the greenhouse.

Yes, working in the greenhouse with seedlings, it was possible to work with very large numbers of plants and to inoculate them successively until a very high percentage of the plants were infected. The small numbers remaining free of infection could be grown to maturity and seed produced for retesting. This was started in the winter of 1936–1937 and approximately 21,000 seedlings were grown in greenhouse benches under covers to provide favorable moistures and temperatures and were inoculated with spore suspensions.

As the seedlings became infected, they were removed from the benches and successive inoculations were made at intervals of about five days. In this first group, the seedlings were inoculated nine times over a period of about two months.

Not much chance of escaping under those kind of conditions, I would guess.

At the end of two months only about three percent of the seedlings still remained disease free. These were then transplanted and were grown for seed in isolated locations. The seed from these selections were then taken back to the greenhouse and compared with the parental material. In first test, where the parental material showed sixty-four percent downy mildew infection, the seed grown from selected plants showed only thirty-seven percent. This was encouraging, but to get the degree of resistance that we wanted, the seed from selected plants was then reinoculated the following year and this was repeated through three or four generations.

To avoid the possibility that we would be selecting for easy bolting characteristics—that is, seed formation at an early date in the growth of the sugar beet—we eliminated all of the plants that produced seed during the first season and all of those that produced seed early in the second season.

So simultaneously you were selecting for both disease resistance and bolting resistance.

Our primary intention was to select for resistance to downy mildew, and we were trying to avoid the possibility of increasing the bolting tendency.

At least maintain the bolting resistance that the variety already had.

Yes, in practice, it turned out that when these selections were taken to the field for testing, the selected material showed a much lower bolting tendency than the parental variety.

Over a period of five years of selection, we inoculated 175,000 seedlings and this included about 13,000 reinoculations of selected materials.

When the best selections from US 33, a curly top resistant variety produced by the USDA, were planted under field conditions in 1939 at Salinas, the commercial variety showed twenty-seven percent downy mildew infection, whereas our best selection showed 9.8 percent and the yield of the selected material was quite a bit better than the commercial variety, primarily because of a much lower bolting tendency.

COOPERATIVE RESEARCH WITH THE USDA, 1940

By 1940 we had clear evidence that the method of seedling inoculation was a satisfactory method of improving the resistance to downy mildew. In discussion with the sugar companies and the USDA, it seemed very desirable to turn over the selected material to the USDA so it could be incorporated in their breeding program.

Who were the people involved with the USDA at that time?

Dr. [Eubanks] Carsner who was stationed at Riverside was in charge of the breeding program for the USDA in the western United States. And I believe Dr. [John] McFarlane had recently come to the Salinas station as he was the one who continued to work with the selection for downy mildew resistance.

At that time, I remember that Dr. Carsner in a meeting with Dr. Gardner and two or three others . . . .

Dr. Gardner was then chairman of the Department of Plant Pathology?

Yes, he was the chairman, located at Berkeley. And he was also over the Davis division of the department. At this meeting Dr. Carsner suggested that since the material was being turned over to the USDA it would be very desirable to have the entire story of selection and comparisons published.

Did you ever do this, Lysle?

Yes, I had no choice. I remember that Dr. Gardner's comment was that: "It appears that you don't object to us leaving the baby on your doorstep, but you do insist on a birth certificate." So the birth certificate was a publication in *Hilgardia* [XVI, 1945, pp. 317-334] that covered our work in the comparisons and varieties, evaluation of the effect on productivity and the story of selection for better downy mildew resistance within some of the curly top resistance varieties.

And I suppose in this publication you detailed the methods you used for selections so that this could go on and be used in additional work.

Yes, this is right. Dr. McFarlane continued to use the seedling inoculation method and since about 1956, all of the varieties developed by the USDA have been inoculated with downy mildew and carry a high degree of resistance.

We seldom see downy mildew in sugar beet fields anywhere in California today. In fact, I can only remember over my lifetime in sugar beet work just a very few infected beets, so you must have done a pretty good job.

Yes, I think this has been a very successful program. In about 1943 Dr. Carsner notified us that after evaluations they had arranged for the multiplication of our best seed selection with the idea of introducing it as an improved strain of US 33. However, new varieties appeared in their breeding program and . . . .

These varieties had higher degrees of curly top resistance, I suppose?

The primary improvement was an increase in curly top resistance, but they were also carefully selected for low bolting tendencies and for other favorable characteristics.

Before 1950 most of the commercial varieties had very little resistance to downy mildew. In 1952 the first moderately resistant variety, US 75, was released and by 1956, the mildew susceptible varieties were completely replaced for use in the coastal valleys with varieties that were highly resistant to downy mildew, each having gone through the rigorous selection procedure. Downy mildew after that became quite insignificant and, as you mentioned, you rarely see sugar beets infected with downy mildew in California. Later as the monogerm hybrid varieties were developed, downy mildew resistance was also introduced into each of the parental lines of these hybrids. In California downy mildew remains only a minor problem. However, in Oregon where some of the sugar companies operating in the Inter-Mountain States and in the East are increasing seed of their varieties, sometimes downy mildew is still quite severe. It's quite clear that the improvement has been from the selection for resistance, because the disease still is present in some areas and potentially could again become a problem if susceptible varieties are used.

So this means that in developing new varieties we have to be reasonably sure that they maintain this resistance to downy mildew, or we could be in trouble again.

·XII·

SCLEROTIUM ROLFSII
(ROOT ROT)

We have a pretty good story on downy mildew and your early work in California in connection with sugar beets. Over the same period I imagine there were other problems that cropped up that you had to do some work on. One of these, I know, was the very important fungus disease of sugar beet: sclerotium root rot. Could you tell us how you got involved in that; first of all, about when was this first noted as a big problem and where?

Well, in 1931 we recognized that some fields in the Sacramento Valley were seriously affected by a root rot disease which is called southern sclerotium root rot. This root rot is caused by a soil fungus, *Sclerotium rolfsii.* The last name means the sclerotium of Rolfs, named after a Professor [Fred Mass] Rolfs, a plant pathologist in Florida who had studied this fungus extensively on several crops. It's called southern because it is favored by very high temperatures and cannot survive extremely low temperatures during the winter. Therefore, it occurs on various vegetables and field crops along the eastern seaboard primarily from Virginia,south to Florida, all across the South and on the West Coast as far north as southern Oregon. This fungus is recognized by an abundance of white mycelium which is a vegetative body of fungus that invades the roots and eventually kills the plants. During its growth, structures are formed which resemble mustard seeds in size and color. They are formed on the mycelium and these sclerotia will live in the soil for several years and are the primary source of inoculum for infection each succeeding year. It has a very wide host range, and had been reported in California on several different hosts, but had really not been recognized as a serious disease on sugar beets.

A NEWLY IDENTIFIED CALIFORNIA DISEASE, 1932

In June 1932, Warren Norton, the Yolo County Farm Advisor, brought in a large bag of rotted sugar beets for identification. I was familiar with the disease on vegetables and some other field crops but had not seen it previously on sugar beets. It was very easy to recognize because of the type of mycelium and the production of large numbers of sclerotia, which are white when very young and gradually turn tan, then dark brown as they mature.

Where did he find these beets?

These came from a field belonging to Mr. Morton in the Clarksburg district. It seems that during the year before they had lost a fairly high percentage of the beets in this field, but had been told that this probably resulted from an excessive amount of irrigation water and that had brought about this rotting of the beets. It was therefore considered safe to replant the field to sugar beets providing they avoided excess watering.

Did they replant it to sugar beets?

They did replant it and the next year, even though they were very careful with the watering, the disease was much more severe than in 1931. So the grower was quite exercised about this, wanting to know what was causing the problem and what he could do about it.

Dr. Kendrick suggested that I investigate this problem. So I first undertook preliminary surveys in beet fields all through the Sacramento Valley. In this I had excellent cooperation from the sugar companies.

Speaking of the sugar companies and how you cooperated with them, when I came to work for Spreckels in about 1942, I had an interview with George Wright who was the district manager in Sacramento and we got talking about the various problems and people on the Davis campus who had been helpful. His comment to me at that time was: "If we had more professors like Lysle Leach working on sugar beet problems, why we'd be better off and wouldn't have so many problems." So he must have thought a lot of the way you cooperated with him.

This was mutual because George Wright was very helpful, first in directing me to fields where sclerotium root rot occurred, and in encouraging his field men to bring in samples or to let me know anywhere that this disease occurred. We were rather startled to find that this disease was widely distributed in the sugar beet fields bordering the Sacramento River, from Colusa on the north to Walnut Grove in the south. Sugar beets seemed to be a particularly favorable host for this fungus. First of all, it was very susceptible to infection by the pathogen and the sugar beet served as an excellent source of food for the fungus, which grew abundantly and produced enormous numbers of sclerotia which were the means of spreading the disease within the field and to other locations.

There was one area, Reclamation District 1660 in Sutter County, where the sugar company had been growing sugar beets for several years in the same fields. There the disease was known as harvest rot because if the sugar beet crops were left in the field after early July, they were almost totally destroyed. So they harvested the fields very early in order to recover at least a reasonable harvest of beets.

Then not only did you lose root yield because of the effect of the fungus, but you lost root yield because you had to harvest so early.

This is true. One of the factors that entered into the severity of the root rot was that many of the leases at that time were for a three-year period and the sugar companies were growing a considerable amount of their own crop because of the severity of curly top and relatively low prices.

In other words, individual growers weren't too happy about growing them at that time.

They were not growing as large an acreage as was necessary to supply the sugar factories. So the companies themselves had to go into farming in many areas and this was usually on a three-year lease which would mean three successive crops of beets. Usually the first, or even the second crop would have very little rot, but by the third year inoculum had built up to a level that caused very severe losses. Then there was no choice but to abandon the field for sugar beets and to move elsewhere.

When we saw the abundance of the disease in this area and the severity of losses, we met with representatives of sugar companies. In 1932 there were two companies operating in this area, Spreckels Sugar Company, and the Holly Sugar Corporation.

At this time Spreckels would not have had a refinery at Woodland. They would have had one at Manteca, though.

This was about five years before the plant was built near Woodland. The sugar beets for Spreckels were shipped either to Salinas or to Manteca for processing.

Manteca, yes. Holly had a plant at Hamilton City and at Tracy at that time.

Yes, Holly had sugar factories at Tracy and at Alvarado. Their plant at Hamilton City had been closed for a number of years but had recently been reopened.

So Alvarado, Tracy, Manteca and Hamilton City plants were then operating.

Yes, Clarksburg was not built at that time. It was built a little later by the Amalgamated Sugar Company and then was turned over to American Crystal. Also in 1932, Mr. Meredith, who was the chief chemist for Spreckels Sugar Company, made a tour with company field men through the Sacramento Valley and he reported on his return to Davis, saying, "This disease is so serious that if we do not learn to control this root rot, it will mean the end of the sugar beet production in the Sacramento Valley." He concluded that work had to be stepped up and that the research that had been started here at Davis offered the best hope for solving this problem. So he contacted the Holly Sugar Company, which was also growing beets in this area, and a meeting was arranged with university plant pathologists.

In this meeting with representatives of the sugar companies, we discussed what we had found in the surveys and it was clear that a comprehensive investigation of this disease was necessary in order to understand how the disease was causing so much damage, how it was spread from one field to another, and what could be done about the problem to minimize the disease's effect. I remember that Mr. Meredith again expressed the opinion that unless this disease could be controlled it would be impossible to continue sugar beet production in the Sacramento Valley.

Following the meeting, the company representatives expressed an interest in supporting research and decided that the work should be centered at Davis. The two companies contributed to a fund which provided about five thousand dollars a year to the Department of Plant Pathology for research on this problem. This grant was renewed annually for five years.

Who was the chief investigator?

I was in charge of the project under the direction of Dr. Kendrick. Five thousand dollars at that time represented a good deal of money and it was possible for us to hire Dr. [A. E.] Davey who had recently completed his Ph.D. at Berkeley. It was during the depression and he was unable to find a position in an experiment station so he was glad to accept a temporary appointment on this project. We also hired a number of students part-time during the school year and one or two of them full-time during the summer to carry out the field plots and the studies that were necessary to answer some of the questions.

Hiring a full-time Ph.D. and several students all on five thousand dollars must have meant that the salaries weren't too high back in those days. Give us some idea of what people were making in professional jobs then.

Well, you must recognize that this was during the depth of the depression, and the starting salary in professional jobs was between two thousand and twenty-five hundred dollars for the annual salary. Students at that time were working for thirty-five or forty cents an hour.

That contrasts rather sharply with—we pay students somewhere around from four to five dollars an hour now.

That's right. Not only were we able to hire this additional assistance, but we had money enough to buy a pickup truck that was used in most of our field plot work.

The first thing we needed to know was how this fungus developed in the field, how it increased within the field and how it could be spread to adjacent areas or over a longer distance. It seemed to me that the most important need was a method of measuring the population of the fungus in the soil and then to determine what happened when the crops were replanted to sugar beets or when the fields were planted to nonsusceptible crops.

We found that when soil samples were washed through a series of screens, we could recover the sclerotia, count them and determine their viability by a germination test on peat soil. With this technique, we could measure the average population in a field, or portion of the field by collecting samples and extracting the sclerotia.

We learned that when a full grown sugar beet was rotted by this fungus, it produced from ten to twenty thousand sclerotia. These were produced on or near the rotting beet. When a field showed twenty-five percent of the beets rotted by the fungus, as many as fifty to a hundred million sclerotia per acre were produced. This meant that samples of soil from that field would contain a considerable number of sclerotia.

We also found that about eighty percent of all the sclerotia were in the top six inches of soil so it was not necessary to sample deeper in order to get a fair assessment of the population of sclerotia. We found that in one year after the production of the infected sugar beet crop, the number of sclerotia would be reduced by as much as fifty or seventy-five percent.

In other words, one year after that crop was harvested, the sclerotia left in the soil would be reduced about fifty percent.

Yes, there was a natural mortality that occurred within the next few months after the harvesting of the crop. And this suggested that rotation with nonsusceptible crops should be quite effective. So we followed population trends where samples of soil were taken from the fields at intervals of perhaps six months and we found that in most fields, in the absence of a susceptible crop, the population could be reduced by ninety or ninety-five percent within a three-year period.

One of the things we were particularly interested in was how the fungus was being spread from field to field, because each year there seemed to be a much wider distribution. We found that when beets were harvested and loaded into the field trucks, large numbers of sclerotia were contained in the soil adhering to the harvested beets. When trucks with loose fitting boards were being used to haul the beets, some of the sclerotia were scattered through the field and onto the roads on the way to the beet loading station. Then when the beets pass over the elevator to be put into cars, fairly large numbers of sclerotia were present in the soil that was screened from these beets. At that time all of the screenings, or tare, from the beets delivered to the beet dump was returned to growers' trucks and they hauled it back to their fields and would shovel it out as they entered their field before collecting another load of beets.

An ideal scheme for spreading the fungus.

Yes, because screenings from a certain truck of beets would not necessarily be returned to the same truck. Very often part of the screenings were delivered to the following truck.

In other words, when Farmer A delivers his beets to the beet dump, the dirt screened from these beets is not all collected by Farmer A's truck. Farmer B may also get some of the same dirt in his truck.

This is very true. Particularly if some of the trucks were coming from infested fields and some from clean fields, this provided a very easy manner of introducing the sclerotia into new fields. We located several fields where the first infestation occurred along the road where the screenings had been shoveled from the truck. Then this infestation over the next few years would gradually spread across the field and produce a completely infested field.

With the result of all these findings, what did they do about handling this tare dirt?

A meeting was held in Sacramento with representatives of the state Department of Agriculture which is the regulatory arm of the state government, and with representatives of the sugar companies. I explained how the sclerotia were being distributed during the harvesting operations.

What would be the approximate date of this meeting?

STATE DEPARTMENT OF AGRICULTURE REGULATION, 1934

This would be about 1934. The state Department of Agriculture immediately established a regulation that no screenings from a beet dump could be returned to the growers' trucks but must all be collected after weighing in a truck used only for this purpose, and that all screenings would be deposited on nonagricultural land near the beet loading station.

At that time, Lysle, to digress just a minute, in all these fields, did you notice any sugar beet nematodes when you were looking at these for sclerotia?

Not during the first few years, but in about 1938, we did run into sugar beet nematodes in a field just northeast of Woodland. And it's my understanding that this is the first case where sugar beet nematode was known to occur in the Sacramento Valley.

So by that time you already had this ruling in place where they couldn't take screenings back on agricultural land. If this hadn't been in place when the nematode arrived, it would have been spread much more rapidly and more thoroughly all over the agricultural area of the valley, I'm sure.

Yes, I think it was quite fortunate that this regulation was in effect because it was effective in reducing the probability of moving sugar beet nematode or weed seeds as with the sclerotia of this root rot fungus.

During our observations on this general subject we also found that large numbers of sclerotia were being carried with beets that were put into railroad cars. And when these were unloaded at the sugar factory, the beets were washed and carried into the factory by means of flume water. By screening the flume water, we found locations where sclerotia were being washed from the affected beets and were carried with the flume water and dumped directly into the river, or in one case into the main ditch of an irrigation district. This would mean that sclerotia could be readily carried for long distances and when water was taken from these sources, could easily introduce enough to start an infection in various fields.

The state Department of Agriculture then issued a regulation prohibiting the dumping of flume water directly into irrigation sources and settling ponds were established at each of the sugar factories. After allowing time for the sclerotia to settle out, the water was passed through screens before it was returned to the river or other irrigation sources. So from these regulations, we minimized the chances of spreading this fungus from infested fields to uninfested fields in the area. I think that this was very important in limiting the distribution of the sclerotia.

I would say that one finding like that all by itself probably paid your salary for all the time you worked for the University, in fact, five or six people like yourself.

Well, I would hope that this is true. I think one of the essential parts of our studies on this fungus was to minimize its spread.

Another question that was raised related to the possibility that sheep or cattle which were then commonly fed on the tops and remaining roots in the field after harvest might carry the sclerotia either externally or internally when they were moved from one field to another. There was some evidence that new infestations in certain fields had resulted from livestock moved from an infested area into a noninfested area. We undertook a study of this question in cooperation with Dr. Tom [Thomas] Mead of the Department of Animal Husbandry at Davis. He provided several steers and sheep and I provided large quantities of sclerotia which were mixed with their feed. The feces were then collected from these animals and washed through screens to determine whether the sclerotia would pass through the digestive tract of sheep or cattle and remain in an intact condition, and whether these still would be viable. We found that the sclerotia began to appear in the samples about forty-eight hours after they were fed to the sheep or cattle. Most of the sclerotia were destroyed during the digestive process, but a small percentage of the sclerotia were collected in an intact condition and a small percentage of these were still viable. It was concluded that if sheep or cattle were moved from an infested field, into a noninfested field, it would be possible to introduce viable sclerotia and thus to initiate an infection in future crops. We found, however, that after four days no discreet sclerotia were found.

So it would probably be safe to move the cattle at that time.

Our conclusion was that after about four days it would be safe to move them into a new area without taking any chance of introducing the disease.

How about the life of the sclerotia that were collected in tare dirt that were piled in various piles in nonagricultural areas? Did they last a long time in a viable condition?

When the screenings were put into a pile, the decomposition of the organic matter caused a lot of heating and this temperature was high enough to destroy all of the sclerotia that were in the interior of this pile. However, near the surface the temperature was not high enough to kill the sclerotia and, in fact, many of them germinated and produced additional sclerotia near the surface. So to get complete elimination, it would either be necessary to cover the piles of screenings or to treat in some other way to avoid the possibility that this material might be transported into agricultural areas.

Lysle, to digress a moment, you mentioned that Spreckels Sugar Company built a refinery near Woodland in about 1937. Were these disease problems you were working with a consideration in locating that plant in Yolo County?

At the time that site was selected for a factory, there were no fields within four miles known to be infested with sugar beet nematode or with sclerotium root rot. When [J.] Earl Coke, then an employee of Spreckels, was making a survey preliminary to location of this factory, he asked me to ride with him to help check a number of fields for possible infestations. Nothing was found within several miles of the ultimate site. Unfortunately, a few years later both sugar beet nematode and sclerotium root rot were found in this area. Earl Coke was formerly an agronomy extension specialist at the University and had worked closely with the sugar beet industry for a number of years. When he went with Spreckels in about 1935, one of his first assignments was to locate this new factory. Later he was promoted to vice president in charge of agricultural operations, and at one point, in about 1943, he offered me a position with Spreckels.

He did? In what capacity?

In the research department. It was an attractive offer, financially, but after careful consideration, I decided to remain with the University.

Lysle, from my point of view, it is a good thing you didn't take that job. I don't think you would have been able to do all the kinds of research you did if you had worked for one company. In addition, think of all the students who wouldn't have benefited from your teaching and administrative abilities at the University.

CROP ROTATION, DISEASE PREDICTABILITY, FERTILIZATION TECHNIQUES

Okay, Lysle, in talking more about Sclerotium rolfsii, *let's discuss the susceptibility of other crops to this fungus.*

It was evident from observations in the field that we could not continue growing sugar beets successively in fields that were infested with *Sclerotium rolfsii.* In order to plan a successful rotation we needed to know what other crops were susceptible to this pathogen and what crops could be used in rotation. For this we needed a plot of land under our control. So I went to George Wright of the Spreckels Sugar Company and explained our needs and he called up Mr. [William] Dwyer, who was the president of the Delta Steamship Line operating out of Sacramento. He [Dwyer] offered us the use of a five-acre block of land on the west side of the Sacramento River about six or seven miles south of Sacramento. Here we laid out plots where a number of the crops grown in this area could be planted over a period of time and we measured the effect of different levels of disease upon these crops and the effect of the crops on the production of sclerotia.

I guess, Lysle, you selected this field because previous observations indicated a lot of sclerotium there.

Yes. The previous year there had been a very heavy loss. Mr. Dwyer did not plan to grow beets in that field for some time and his contribution was to let us use the land free of charge for a period of five years. In this field and from other observations, we were able to establish that some of the most susceptible crops in addition to sugar beets were carrots, beans and potatoes. We also determined that among the most resistant to infection were alfalfa, barley, corn, asparagus, and that because of temperature limitations, all winter grown crops escaped infection. With alfalfa, we had very good success in reducing the inoculum, but in a few fields where weeds were abundant, the pathogen grew and multiplied on the weeds and tended to maintain the population. In one field, after four years of alfalfa, the population of the fungus was very low and when it was replanted to beets the following year, we found less than one percent of infestation. So that would represent a very successful rotation.

We also needed to know more about the population of the fungus in growers' fields, so we developed a system of collecting the samples from commercial fields. And we set up a laboratory on campus where two students, Wayne F. Weeks and Mac D. Osborn, were employed to wash soil samples through screens of appropriate size to screen out the sclerotia. We could then count the sclerotia with reference to the weight of the sample, and we could measure their viability in the laboratory. In this way, we could take a field that was to be planted with sugar beets, measure the average population of the sclerotia, and then during the summer, we would follow the crops and carefully record the percent of roots that became infected at different periods through the summer.

After harvest was over, we would again take samples to measure the population of sclerotia to see how big an increase had occurred as a result of the percent infection in that field. Over the next three or four years, we would take samples once or twice each year to determine how rapidly the sclerotia would die and disappear and how long we would have to grow that field to nonsusceptible crops before it would be safe to replant sugar beets.

What was the approximate time period, Lysle, when you were doing these studies?

This would be from about 1934 to around 1939. With this information, we were able to plot the relation between the population of sclerotia in the field before planting of sugar beets and the percentage of infection that would be anticipated at certain intervals during the summer. We were also able to establish the multiplication of the sclerotia on sugar beets during the period of infection. We were fortunate that we found only one infected beet for about each thousand sclerotia present in test fields. With this system and the experience of several years, we were able to take samples before planting of sugar beets and to predict the approximate level of infection that would be expected to occur.

Over a period of about ten years, we analyzed samples from about twenty-five thousand acres and of this acreage, approximately one-fourth was so heavily infested that if the grower had proceeded with his intention to plant sugar beets, he would very likely have had losses exceeding twenty percent, which we assumed was beyond the limit that would provide a profitable crop to the grower.

Lysle, this is such an important thing—this idea of sampling the soil to determine the possible loss when you grew a sugar beet crop. Many of the readers may want to know where this information can be found. What sort of publications did you produce that would contain this kind of information?

The primary publication covering this material was in *Journal of Agricultural Research*, published by the federal government in Washington, D.C. This paper was published in 1938. Popular articles concerning it appeared in a number of the sugar company journals, and in local releases.

The Journal of Agricultural Research *in 1938 would be the place to find that [56:619–631].*

Unfortunately, this publication was discontinued a few years later so it is not a current journal series.

At that time, I believe this was the only system of taking samples from growers' fields in advance of planting and predicting the losses likely to occur.

I think it's interesting to note that in this modern era of integrated pest management, this seems to be one of the key techniques that many researchers are trying to develop for other crops.

Well, the nematologists have been very successful in developing systems of sampling field soils and tare dirt from loading stations so they are making similar types of predictions with reference to the sugar beet nematode.

So Lysle, after finding this procedure and putting it into commercial use for predicting losses from this fungus and learning what kinds of rotations to reduce losses, what did you do next?

Well, that provided the basis for rotation systems but we still needed some method that would more directly reduce the amount of infection. And this came in the area of nitrogen applications. When I started this work I knew of research in Arizona where plant pathologists had shown that the cotton root rot disease could be partially controlled by adding heavy applications of manure or spoiled alfalfa hay to the fields. In a similar way, Dr. Lester Blank, then at the University of Wisconsin, had shown that by applying calcium cyanimide to soils, he could significantly reduce the amount of club root on cabbage. Both these findings seemed very interesting and I decided to try them both at the Lisbon plots which were under our control and in some growers' fields.

The Lisbon plots were the ones just south of Sacramento.

Yes, Dwyer's field. We started out the first year by applying manure in heavy amounts. We also applied lime because the fungus *Sclerotium rolfsii* is favored by acid soil conditions and we hoped might be inhibited by lime. We used the calcium cyanimide, which carried some lime and also in its decomposition produced ammonia. And for comparisons we made applications of ammonium sulphate to various plots. We did not get any appreciable reduction in disease from manure or lime, but we found a very significant increase in growth of beets from all of the applications that provided nitrogen fertilization. That included ammonium sulphate, ammonia and calcium cyanimide. We also, in the same year, set up plots in growers' fields where different rates of nitrogen were applied, ranging from fifty pounds to about two hundred pounds of nitrogen per acre. Additionally, we compared the anhydrous ammonia dissolved in the irrigation water with ammonium sulphate, also applied in the irrigation water or side dressed to the plants.

At that time nitrogen fertilization was not very common with sugar beets. It seems that early in sugar beet production in the Sacramento Valley, they had tried fertilizer applications at very low rates, not exceeding about twenty-five pounds of nutrient per acre, and did not see enough of a response to believe that it was worthwhile.

With these comparatively high rates of application of nitrogen, we got very strong growth response of the sugar beet and were somewhat surprised, and very favorably so, because the sclerotium disease was very slow in developing and even at the end of the season showed much lower percentages of infection than in the untreated plots.

In other words, fewer beets were actually infected when you had good nitrogen nutrition.

Yes, that is right. We followed up on this with additional plots in the successive years and almost without exception, the more nitrogen that was applied, the lower would be the percentage of infection. Yields were increased for two reasons: one was the plant response to the fertilizer and the other was the saving of plants by the reduction of disease.

As an average in fourteen separate field experiments conducted over a period of four years, we found that fifty pounds of nitrogen would reduce the loss to about seventy-two percent of the disease in the check plots. With one hundred pounds of nitrogen the crop loss was reduced to about forty-six percent and two hundred pounds of nitrogen reduced the plant loss to about a thirty-five percent level.

Now it developed that two hundred pounds of nitrogen was probably more than desirable because the beets matured slowly and in some fields this resulted in reduced sucrose percentages. But one hundred pounds of nitrogen represented a very economical application and was only a little less effective than the two hundred pound rate in terms of reducing disease.

In your opinion, do you think this effect of nitrogen is because Sclerotium rolfsii *doesn't like this nitrogen or because of the effect it has on the sugar beets, making them somewhat resistant or something of this sort.*

We carried on a good deal of work to try and determine why this was working. Our first observations were based on ammonium fertilizers. Laboratory tests showed that ammonia was a very good fungicide and, at one point, we thought that we were controlling the disease because of the ammonia that was applied or that resulted from the decomposition of the calcium cyanimide.

We found, however, that when applied as anhydrous ammonia, it did not penetrate the soil as ammonia far enough to cause any significant reduction in the viability of sclerotia or to prevent their germination. To test this point, we tried calcium nitrate at the same nitrogen level as ammonia and ammonium sulphate. In some fields the calcium nitrate was even more effective, and in other fields it was about equally effective. Therefore, we had to conclude that the effect here was not the direct effect of ammonia on the fungus. It was obvious that there was a great plant stimulation and that it was possible that by increased growth of the beet roots that the fungus would be in some way prevented from invading or infecting the roots.

There was also the possibility that application of the nitrogen could stimulate other microorganisms in the soil, and in some way, these other microorganisms may have interfered with the development of the pathogen. In any case, it does not appear that the materials we applied were directly fungitoxic to the sclerotia.

I'm sure the farmers didn't really care about all that. The main thing they were interested in was what they could do to reduce the effects of the disease, and you certainly showed that.

About the same time [1932–1936], Dr. Raymond Pendleton, an agronomist working with the USDA, was located at Davis and he conducted a number of field trials using various forms of fertilizer. He showed that applications of phosphate and potassium had very little effect, but that applications of nitrogen in the range of one hundred to a hundred and twenty-five pounds per acre were very effective in increasing the yield of sugar beets.

The third point was that under the sugar beet production program at that time, growers were required to apply certain fertilizers to their sugar beet fields as a means of complying for benefit payments. So with these three points working together, that is the fertilizer benefit, the disease control effects, and the need for compliance, fertilization with nitrogen was almost universally accepted within two or three years. The tendency at that time probably was to apply more nitrogen than was needed. This resulted in some problems involving quality of the sugar beets for processing. Consequently, over the years, the amount applied has been reduced from the maximum rate and much more precise methods of measuring the amount of nitrogen actually needed have been devised.

I think it's very interesting that at this time you apparently really corrected two problems. You didn't start out to correct the nitrogen fertilization problem, but that seemed to be the result as well as controlling the disease, so it was a very productive piece of research, I'd say.

Yes, we were quite satisfied with the results because it gave us a method of directly influencing the amount of disease.

COOPERATION WITH SUGAR BEET GROWERS

To do this work we needed the help of a great many sugar beet growers. One that I would particularly mention was William J. Duffy, or Bill Duffy as we knew him, who was in the Sutter Basin area as the manager of the Sutter Basin Corporation's farms. Whenever we needed a plot, he gladly let us run our experiments in his fields, or he would locate a farmer who had a plot of land that would be most suitable for our trials. And it was this wonderful cooperation from people like Bill Duffy and other sugar beet growers, as well as the employees of the sugar companies, that made it possible to run such extensive field experiments. Later on, when we were working with seed treatments, some of the earliest trials were conducted on fields belonging to Bernell Harlan, south of Woodland.

Both Bill Duffy and Bernell Harlan had been students at Davis, hadn't they?

Yes. And later when we were working with combinations of insecticides and fungicides and with some of the newer combinations of fungicides for seed treatment, Chet Locke, another Cal Aggie, was very helpful in providing plots where these could be tested under field conditions. I would say that Chet Locke was probably the first sugar beet grower in California to make commercial plantings with mixtures of Dexon and PCNB and later mixtures of these fungicides with lindane in our cooperative trials with Harry Lange from entomology. Chet is located at Lockeford, directly east of Lodi, along the Mokelumne River [in San Joaquin County].

So you did a good bit of traveling to get to all of those places?

We traveled a good deal and particularly when we were working with trials with lima beans—large lima beans—which were grown primarily along the coast between San Diego and Santa Barbara and trials with cotton which were primarily in the southern portion of the San Joaquin Valley.

Well, what comes next in the story of Sclerotium rolfsii?

Well, we tried a number of other approaches. Agronomists working with the USDA attempted to select for resistance by collecting beets in fields where very high percentage of infection occurred. Charlie Lavis, with the USDA located at Davis during the early thirties, started this work and Gus Larmer who succeeded him at Davis, continued it. Later, some of the companies also made selections. But up to now, no really satisfactory resistant varieties are available.

We tried at one time to locate fugicides that could be used to eradicate this fungus when it was found in a localized portion of a field. Dr. Davey took responsibility for testing various fungicides and for trials in growers' fields. But, in general, we were unable to locate a fungicide that would give complete eradication and, in any case, what we tried would have been so expensive that it really would not have been useful. We have no case where we completely eradicated the fungus after it was established in the grower's field. And in nearly all cases we found it much more widely distributed than it was first thought, so eradication really was not practical.

As a result of the information that was collected over a period of about seven years, we found no single measure that could be said to be the control for sclerotium root rot. But we were able to develop information that led to minimizing the spread of the fungus, particularly from field to field, and by careful selection of crops, it was possible to carry out rotations that would reduce the amount of infection so the fields could again be planted to beets.

With the sampling technique, it was possible to measure the severity of infestation before a grower made the final decision whether to plant sugar beets or not, and thus avoid the planting of fields that were doomed to failure. With the use of nitrogen to promote maximum growth and minimize the disease effects, the sugar beet growers were able to continue growing beets in the Sacramento Valley and the production of sugar beets has increased both in area and in yields.

At the present time the disease still occurs in a few fields each year, but in general the losses are light to moderate and not enough to threaten the industry.

Well, Lysle, that was an extremely interesting story about Sclerotium rolfsii. *There must have been some other diseases of sugar beets that you learned about while doing this kind of work. What were they?*

XIII

SEEDLING DISEASE RESEARCH

One of the things that I frequently ran into was that growers of sugar beets would call my attention to the fact that they were having difficulty in getting good stands of sugar beets even though they were using as much as fifteen or twenty pounds of seed per acre. This rate should have provided far more seedlings than needed, but in certain cases very few seedlings would emerge. In other cases the seedlings would die after emergence.

That rate of seeding would probably provide a seed every inch or even more.

At least a seed every inch, probably a little more than that. And since the growers wanted to end up with a plant every foot, this was a great many more than they needed or wanted.

COMPLEX OF FOUR INTERRELATED PROBLEMS

So something had to happen to a lot of seedlings.

Yes. There were indications that certain soil borne pathogens were attacking the germinating and emerging seedlings. I collected soil from several fields, brought it into the greenhouse, planted it to sugar beets and observed the results. In many of these cases, I found soil fungi that were rotting the young seedlings and preventing their emergence or resulting in death from what we term post-emergence infection.

I also tried some seed treatments but the list of fungicides available at that time was very limited. With cereals they were using such materials as copper carbonate, or the then new organic mercury compounds, such as Ceresan or New Improved Ceresan. When I treated the seeds with these materials, I found that the copper carbonate was quite ineffective but the organic mercury compounds were very effective in preventing these seedling diseases.

I also tried other protectants that had been used with different crops and found that either red or yellow copper oxide was quite effective in controlling the seedling disease. The next step was to try these seed treatments in field plots. So I waited until a grower had planted a field and lost his stand because of what appeared to be seedling disease, and then in the replant I would get permission to set up plots where the seeds would be treated with different fungicides at different rates. Some of these field plots were quite spectacular and it attracted a great deal of attention in the sugar industry.

In one case, in a field plot on King Island just north of Stockton, we found that using US 33, a new curly top resistant variety, it didn't matter whether we treated the seed with copper oxide or with the mercury compound, Ceresan, because in both cases we had very good control and a very successful stand in the replant. On the other hand, the nontreated plots again showed completely unsatisfactory stands. In the same field, however, we used a variety [Hartmann], imported from Denmark, and with this variety, the mercury compound was very satisfactory for controlling the disease, but the copper oxide was not.

That's kind of a strange situation. What was going on there?

It seemed a little strange to us to have seed treatments varying in results according to the varieties of sugar beet. But when we examined the seedlings and made cultures from them, we found that the European seeds carried a heavy infestation of a seed-borne fungus called *Phoma betae.* This fungus develops on the seeds grown where there is an abundance of rainfall during the growing season.

In other words, when the seed is being produced for the farmer to plant, that's when the Phoma grows, right?

Yes, that's right. And in Denmark, where they have a great deal of summer rainfall, this fungus had an opportunity to grow on the flower parts and to invade the beet seed balls. So that when we planted them here, we had the Phoma seedling infection carried on the seed as well as a fungus we call Pythium which was present in the soil. It developed that the mercury compounds were effective against both organisms because they are general biocides, whereas the mild protectants such as copper oxide were effective against the soil-borne Pythium, but not against the seed-borne Phoma.

When we put field plots in other areas, we found another soil fungus called Rhizoctonia which produced a similar disease on the seedlings. This seedling pathogen could be controlled by the mercury seed treatments, but not by copper oxide.

About a year later, we found that in certain areas the seed treatments were quite effective in protecting during emergence, but thereafter, seedling infection occurred whether the seed was treated or not and whether mercury or copper had been used. So this represented a fourth seedling disease type where seed treatments were only effective in aiding in emergence and not in protecting against the post-emergence type of infection.

Why weren't the seed treatments effective against this particular situation?

I think particularly because the fungus which was in the soil would invade through any roots.

This is a different fungus then than the Phoma or Rhizoctonia.

This is entirely different. It is called *Aphanomyces cochlioides.* This fungus lives in the soil and will attack the young seedlings any time after the root hairs develop. This meant then that after the beneficial effects of the seed treatment had occurred, the infection could continue to develop and this would result in a weakening of the stem tissue so that the seedlings very often would fall over. Some would die, others would survive and recover, but nearly always the result was a considerably reduced growth and yield.

What do they call the condition when seedlings just fall over, is there a general term for that?

In many places, this is called black root. This is a little confusing because Phoma infection is often called "black leg." The two organisms are quite different and the effects on the seedlings differ considerably. I preferred to refer to these seedling diseases according to the generic name of the pathogen causing them.

These trials pointed out to us that the seedling disease complex of sugar beets consisted of at least four different seedling diseases, caused by different pathogens, and the measures necessary for their control were quite different. Very often these pathogens occurred in combinations of two or more different seedling diseases.

With the Aphanomyces seedling infection, we learned quite early that this organism would develop only in warm, moist soil. If we planted our sugar beets as early as January or February, the stand would develop to such an extent that they would escape infection. Where it is possible to plant the sugar beet seed early in the spring, this provides a very effective method of avoiding this seedling disease.

One thing, Lysle, how did you know what organisms were involved in these various seedling diseases? They looked pretty much the same from the symptoms. How were you able to tell and help other people tell what organisms were involved?

Well, the system we used was very simple. Usually we would bring the soil in from the field and we would grow a crop of seedlings with part of the seed untreated and part of it treated with our most effective fungicide, which at that time was Ceresan. Then we would take the infected seedlings from the soil in the greenhouse, surface-sterilize the seedlings and place them on nutrient agar in a petri dish and then carefully observe the organism that would grow out from the infected seedlings. As time went on, we found that several of these pathogens could be readily identified by merely taking the seedling from the soil, washing it to remove the particles of dirt and placing it in the shallow layer of water in a petri dish. After twenty-four or forty-eight hours, we placed the petri dish under a compound microscope with a low power objective and we could see the mycelium and the fruiting structures of the fungi so that they could be readily identified from one another.

So that provided a very easy diagnostic technique that most people could use to learn what was causing their problem.

Yes. Later on [A. Mangin], a young lady working in the seed laboratory in Ireland, developed a very simple procedure for identifying Phoma on beet seed or beet seedlings by placing them on water agar and then a specialized type of growth would occur in contact with the plastic petri dishes and it was quite easy to recognize the Phoma as the pathogen merely by placing inverted plates under low-power magnification.

It seems, as you've pointed out, that you learned that there were several organisms that caused seedling diseases, and you learned how to identify them. But it seemed to me that during these investigations you also did some fairly fundamental work with regard to how these different organisms grew in relation to the growth of sugar beet seedlings that had some influence on how to deal with them.

I think there's one point I might go ahead with before I get into that. In the field it seemed at that time that the mercury compounds were perhaps the preferred material to use because they were effective against three of the four seedling pathogens. So we carefully worked out the dosage of the Ceresan compounds and the minimum dosage that provided the best results were used commercially for about two years. We soon found, however, that when sugar beet seed was treated with the mercury compound and stored under conditions where moisture was absorbed that the mercury caused an injury to the seedlings that would be shown by distorted and inhibited germination. We first recommended that no seed treated with mercury be stored for longer than one month, but this was fairly awkward for the industry because they have to treat thousands of pounds of seed and ship it around the state for use.

Later on we found that another nonmercurial fungicide would provide almost as good control for Pythium or for Rhizoctonia. And since these soil-borne organisms were the most common, we decided to discontinue the use of mercury seed treatments for sugar beets and to concentrate on the use of this new material which was called Phygon.

When was this observation made?

Our trials with Phygon were conducted from 1938 to 1940. This material was not phytotoxic to the seed or seedlings, but did cause some skin irritation for some of the operators who were applying the dust to the seed. I might add that after the introduction of curly top resistant varieties, all seed used in California was grown in the arid Southwest and carried almost no seed-borne Phoma, so mercury treatments were no longer needed for this seedling disease.

You mention, Lysle, that the companies were involved in this seed treating. Was that something fairly unique to the sugar beet industry, where the company treated all the seed for the farmers who didn't have to do it themselves, or what was the situation?

Well, I think with most other crops there were commercial operators who treated thousands of pounds of wheat or barley or corn or peas or beans for sale to the farmers as treated seed. But sugar beets were unique in that they were contracted in advance of planting and the companies provided all of the seed. So it was logical that they would set up the seed treatment plant for each company and treat the seed for their growers.

· XIV ·

WORLD WAR II COLLABORATION WITH ROY BAINER

Let me ask you something ahead of time here. What was the time frame when Professor Bainer in Agricultural Engineering and others started getting into segmenting beet seed? I know you helped him in that process and this probably had some relationship to the importance of seed treatments, since they were attempting to plant much lower seeding rates.

Yes, work on seed processing started about 1940. I mentioned that during the earlier period very heavy amounts of seed were planted to make sure that an ample stand would be secured. This required a great deal of hand labor to single the beet seedlings in the field. This was done with a short handled hoe, working from your knees, and in very thick stands it required what they termed finger singling, that is, pulling out the beets by hand so that only one was left at each location, usually eight to twelve inches apart.

As I understand it, it's illegal to use a short handle hoe nowadays.

That's right. That has been outlawed for some time. Fortunately, we do not need to work from our knees anymore for this purpose.

The United States was not yet in World War II, but the war was being fought in Europe and it appeared that we were going to have some difficulty in providing enough field labor to take care of the operations that were necessary to produce a good stand of beets for sugar production.

ENGINEERING FOR SEGMENTED SEED, PRECISION PLANTING

So Roy Bainer, an agricultural engineer, conceived the idea of developing a machine that would reduce the number of germs in a seed ball. A beet seed ball is really a cluster of several flowers fused at their bases, each developing a single seed. So what we planted was called a beet seed ball and it might contain from one to as many as five or six seeds. When they germinated, not all of the seeds produced seedlings, but in some instances, several seedlings could emerge from the seed ball and this made it difficult to produce singles in the field. So Roy worked on this problem and tested a number of methods of reducing the multiple numbers of seeds per unit. We entered World War II in 1941 and then with the labor shortage that developed, it became extremely urgent that we develop methods of reducing the amount of the labor requirement in the field so that we could continue the production of sugar.

Cane sugar imports from abroad were unpredictable and sugar soon became a rationed household commodity, according to Roy Bainer's oral history.

That's right. To help solve this problem, the machine Roy ultimately designed consisted of a horizontally mounted revolving stone fitted so that the seed would be fed down between the revolving stone and a fixed metal shear bar. This had the effect of knocking off the outer cork from the seed ball and would break it into smaller units. These would be screened and cleaned and the units containing one or two seeds were called the segmented seed.

Roy also worked on precision seeding units that would drop single units of segmented seed. With the precision planting units, he could reduce the seeding rate and distribute the seed uniformly so there would be very much reduced labor requirements.

Now it seemed like two things were needed: to make sure that this shearing process isn't damaging those seedlings and also to make sure that there was a good enough seed treatment to protect against these damping-off fungi that you were working with.

The segmented seed served a very useful purpose as he had developed it. It was estimated that instead of requiring about twenty man-hours to single an acre of beets, only about ten man-hours was required. They calculated that something like five million man-hours were saved by the use of segmented seed, as compared with the use of whole seed.

Just in California alone?

No, this would be in the United States. Within two or three years, segmented seed was accepted and used all over the United States. I think only one company did not use the type of equipment that was developed by Roy Bainer here at Davis [in the Department of Agricultural Engineering].

In the field, in spite of the great labor saving, there were some problems. With some planting equipment, it was very difficult to get a full and uniform stand using such low seeding rates. In 1943 Dr. Eubanks Carsner, with the USDA at Riverside, California, and Bion [A.] Tolman, with the Utah–Idaho Sugar Company of Salt Lake City, Utah, reported that seed processed by the segmenting process showed considerable internal injury to the embryos. Emerging seedlings showed fractures of the radical which would result in abnormal roots or injury to hypocotyl or cotyledons, leading to abnormal development of aboveground parts of the seedlings. It appeared that it would be desirable to accomplish the labor-saving benefits without the internal injury that resulted from the seed passing between the revolving stone and the fixed shear bar.

During this period, I worked rather closely with Roy Bainer because with the reduced seeding rate, it was imperative that we use a seed treatment that provided adequate protection to the seedlings in the field. Roy and I often discussed how the method of processing the seed could be modified so we could retain the benefits of reducing the multiple germs in seed units without the internal injury. As I remember it, during one of these conversations I said, "Why don't we rub off the excess cork and germs instead of breaking them?" Apparently Roy liked the suggestion and proceeded to design equipment for testing. In Professor H. B. Walker's paper covering the mechanization of sugar beet production he said, "Leach initiated the idea of using decorticated whole seed, and Bainer developed an experimental decorticating machine." [Walker, H. B., "A Resume of Sixteen Years of Research in Sugar Beet Mechanization." *Agricultural Engineering.* Vol. 29, No. 10, October 1948, p. 427.]

What was that idea?

DECORTICATION OF SUGAR BEET SEED

In the process of decortication, the seed was rubbed and rolled between a horizontal stone revolving at a high rate of speed and a neoprene pad that was attached to a plate mounted at a very precise distance above the revolving stone. The seed then being fed in the center from above, would be rolled and rubbed as it moved toward the outer portions of the stone. Eventually the material came off the edge of the wheel and was collected in the housing and was then screened and aspirated so that dust would be removed. Oversized units would be fed back through the unit. The finished seed consists of smooth units of high density that could readily be planted with a precision planter.

To maintain high percentage of germination and freedom of internal injury, it was essential to test all seed samples in a seed germination laboratory in the Agricultural Engineering Building [Walker Hall]. Our oldest daughter, Joann, who was then in high school, served as the part-time seed analyst in this laboratory.

What was the main difference between the reduction of the number of germs per seed ball in the two processes, the segmenting versus the decortication, that made the decortication somewhat superior?

I think the chief advantage of the decorticated seed was the ability to handle it in a very precise manner during the planting operation, which contributed to the labor saving. The one disadvantage of the decorticated seed was that the average seed unit,after processing was finished, would contain about one and one-half seeds per unit, whereas the segmented seed usually contained between 1.1 and 1.3 seeds per unit.

Well, what about the number of damaged seedlings?

There were essentially no damaged seedlings with the decorticated seed because, with the rubbing and rolling, if it broke through a locule, the entire content of that locule would be removed and would be screened off along with dust and small particles.

Are there areas of natural cleavage between different flowers that are clustered together in a seed ball where it would tend to break when it was gently rubbed like in the decortication process?

That was the hope that Roy Bainer had when he started his segmenting process. But it did not work out that way because the pericarp was a very hard, horny material and it did not readily break into units. It was a case of selective injury by the removal of some of the outer units rather than separating into two or more units. When we began to test the characteristics of the processed seed, we found that the decorticated seed had many advantages over either the segmented or the whole seed, including the ability of the unit to absorb moisture and to germinate rapidly.

In 1945 Roy Bainer was invited to come to England during wartime to advise the British on mechanization of the sugar beet industry. He and Professor Walker asked me to continue Roy's work on seed processing and to supervise field trials during the three months he was in England.

In field trials at that time, it was quite common to observe a low percentage of potential emergence of the seed. Only about fifty percent of the viable germs produced seedlings and in some cases the emergence was even less. Under these low percentages of potential emergence, many of the seed units containing two seeds would actually produce only one seedling. So under field conditions very often the percentage of single seedlings would be much higher than shown by the same seed under laboratory germination conditions. It follows that with very low seeding rates, the probablility of having at least one seedling where you wanted it was much greater with decorticated seed than with segmented seed. This was cited as one of the real advantages of decorticated seed.

At first there was considerable reluctance on the part of some of the sugar companies to accept decorticated seed as a substitute for segmented seed, because they had focused their attention on the fact that it was important to produce as near single germ seed as was possible.

In our view, the most important thing was to prepare a lot of seed of high germination, with high density and a uniform surface so it would feed satisfactorily through a precision seeder, and then to use as low seeding rates as was safe under the given field conditions.

RESEARCH FUNDS TO MECHANIZE THE BEET INDUSTRY

So I suppose this development of more or less single germ seed balls had a great influence on the more rapid development of a precision sugar beet seeder.

Roy Bainer had worked on precision seeding units and, based on his work, a number of commercial companies began to put out commercial units that were quite satisfactory for distributing processed seed in a uniform manner. Some of the sugar companies, as I said, were a little reluctant to switch. They were concerned about the higher percentage of doubles that were present in the decorticated seed. I remember when I was discussing some field trials at a meeting, Mr. Wilford Cannon of Utah–Idaho Sugar Company said afterwards: "If what you are telling us today is correct, then everything we've been doing the last two or three years was wrong, and I just don't believe it." Yet once they had tested the decorticated seed, and used it in field plantings for two or three years, they reported to their growers the reasons why they had switched from segmented to decorticated seed, and they followed very closely the material that we had presented to them in the committee reports at Davis.

Was this project sponsored to some extent by a national organization?

Yes, the whole program of mechanization was sponsored by the U. S. Beet Sugar Association and with all of the companies involved, they collected what at that time [1938] seemed like an enormous research fund. They started with something like eighty thousand dollars and additional amounts were provided later. They looked all over the country before deciding that the ag engineering department at Davis was where they wanted to locate the major part of the research work. Almost the entire fund was made available to the Department of Agricultural Engineering here. The development of processed sugar beet seed, precision seeders and many of the components that went into the machines for harvesting beets resulted from this program that extended over a number of years.

It seems that you really had a hand in a development that was very instrumental in improving the efficiency of the sugar beet industry. This is a good example of how public support of research can really be of great benefit to the agricultural segment of the country and to the general public as well, in terms of having a more efficient industry.

Well, certainly the mechanization that was done here at Davis during that period was a very important contribution. As a plant pathologist, I was a little surprised myself how involved I became in the process and I think it would not have happened if it had not been an effort during the war period when everyone was trying to fulfill whatever was the greatest need. This interdepartmental research project came about because I had been working with seed treatments, first on whole seed and later with some of Roy Bainer's early developments of segmented seed, and continuing into the protection of the decorticated seed later on.

Well, what should be next in the story of sugar beet seed treatment and seedling diseases?

·XV·

CONTINUING RESEARCH ON SEEDLING DISEASES

Well, I'd like to go back and pick up a little more on the theme of seed treatment. I'd mentioned that we discontinued the use of the mercurys because of possible injury and were protecting the seeds by the use of Phygon as a seed treatment. But we had in mind that it would be very desirable to find new fungicides that would be more effective in protecting against the soil-borne organisms.

You were looking for newer fungicides to control seedling dseases.

Yes. We felt that it might be very difficult to find any one fungicide that would control the various diseases that were involved, but it was our hope that we would find new materials that would have a higher level of effectiveness. I had the hope that we could combine two or more fungicides and that this would give us a wider range of protection and a higher level of efficiency.

While I was in Europe in 1951, I learned about a fungicide called PCNB [pentachloronitrobenzine], and found that it was being made available in the United States. I tested it with various organisms and found that it gave a high level of protection against Rhizoctonia, but had little or no benefit with Pythium or Aphanomyces. A few years later, in 1956,while visiting Bayer Chemical Company in West Germany, I learned of a fungicide later named Dexon, which they considered to be very effective as a soil treatment in controlling certain fungi, including Pythium.

I obtained a sample of this fungicide and tested it in the greenhouse at Davis. As a seed treatment, Dexon was very effective in controlling Pythium damping-off. As a soil treatment, it also controlled Aphanomyces, but had no benefit against Rhizoctonia. By combining PCNB and Dexon, we had a treatment that provided very effective protection for seedling diseases caused by either Pythium or Rhizoctonia. In the meantime, with the introduction of new curly top-resistant varieties by the USDA, the use of European grown sugar beet seed was completely discontinued and a domestic sugar beet seed industry developed in Arizona, southern California, and southern Utah. We found that whereas European seed lots frequently carried large quantities of Phoma, the seed grown in the arid southwest showed almost no infection, and therefore there was no necessity for including a treatment for control of Phoma.

DOUBLE PROTECTION, MONOGERM SEED

After two years of testing in the greenhouse and in growers' fields, by 1959 all the sugar companies operating in California started using Dexon and PCNB. This combination seed treatment has been used for the past twenty years in the western states and, according to reports from the sugar companies and the growers' association, has provided a very satisfactory protection against seedling diseases of sugar beet.

Since about the mid-sixties, growers have been planting all genetic monogerm seed at even a more reduced seeding rate, and even with those reduced seeding rates, this combination of fungicide treatments seems to have done an excellent job in maintaining a good seedling emergence.

In about 1947 two Russian sugar beet geneticists, Dr. and Mrs. Savisky, immigrated to the United States. The husband, Dr. V. F. Savisky, had located a genetic monogerm selection of sugar beets in Russia, but was not permitted to bring it to America. Soon after his arrival, the Curly Top Resistance Breeding Committee, with the support of the U. S. Department of Agriculture and the beet sugar companies, set up a project for him to attempt to locate similar genetic monogerm plants in our seed fields. After considerable searching, he found a few monogerm plants in a seed field near Medford, Oregon. This material was incorporated into the USDA variety breeding program at Salt Lake City. True genetic monogerm selections were distributed to sugar beet breeders and within a few years commercially acceptable monogerm hybrid varieties were available in all sugar beet producing areas.

This development eliminated the necessity of processing the seed to improve singleness, but rubbing the seed to remove excess cork tissue is still practised to aid in precision planting. At first the decorticating units were used, but most seed processors now use a rice polisher for this purpose.

In the meantime, Dr. Helen Savitsky undertook a program of interspecific crosses between other species of the genus Beta and sugar beets as a means of developing a high level of resistance to sugar beet nematode. She is still pursuing this line of research at the USDA Research Station, Salinas, California.

So with that and the use of this very effective seed treatment, many growers now plant as few as three seeds per foot, or about four inches apart, and quite a few of them don't have to thin sugar beets anymore.

That is correct. There is one problem that frequently cropped up in the fields when we were testing seed treatments. That was attacks by wireworms and in some cases by cornseed maggot. I attempted for some time to find an entomologist who was interested in working on the possibility of combining an insecticide along with the fungicide as a seed treatment.

COOPERATIVE RESEARCH WITH HARRY W. LANGE, ENTOMOLOGIST

When Dr. Harry [W.] Lange was transferred from the station at Salinas to the Davis campus, I discussed this with him and he was quite interested. At first he tried DDT, but this was too slow acting to be effective as a seed treatment. Then he used a material that later came to be known as lindane. This material was extremely effective against wireworms and cornseed maggot and we undertook field trials with seeds treated with the lindane and appropriate fungicides.

In the south coastal area from Santa Maria on the north down to San Diego on the south, they grew a large acreage of large lima beans. Lima bean seeds are quite susceptible to decay caused by Pythium and to attack by either cornseed maggot or wireworms. So we set up quite a few of our trials in that area. Years ago, an agronomist had tried treating seeds with Ceresan, as we did with cereals, but the lima bean was so sensitive to the mercury seed treatments that this could not be used on lima beans. For a number of years we used a mild seed protectant such as spergon, Thiram, or captan. This provided good control against the Pythium seed decay, but of course no benefits against the insects.

What were the main fungi causing problems with the lima bean seedlings?

Pythium was the most important. Rhizoctonia also infected bean seedlings, but quite a bit later. It was more of a root rot type of infection and was not particularly a seed decay organism.

The main effect of this Pythium then is to rot the seed in the ground and it never emerges.

That's right. You can end up with a very light stand. When we used the combination of insecticide and the fungicide, we got very satisfactory stands. And this combination treatment was quickly adapted by the bean seed growers. It was found that if they used only the insecticide, it seemed to predispose the lima bean to infection by Pythium.

It seems to me, Lysle, that they were having lots of problems with lima bean stands and really didn't know what was going on. What did you do when you found all this out?

Well, we noticed that when the seeds were treated only with lindane, the emergence was lower than with no treatment at all, and in laboratory trials it appeared that the Pythium was more severe on the lindane treated than on the untreated seed. It developed that it was not really a safe procedure to treat the seeds only with lindane but rather in combination with a protective fungicide, like captan or thiram. Within a few years, nearly all lima bean seeds were treated with this combination.

This work that you and Harry did with combination fungicide and insecticides, has it been carried on to any other crops?

Yes. Because of the success of this combination of pesticides used as seed treatments, it was then tried with a number of other crops. Dr. Lange found that he could control seedling insects on a large number of crops, including the cereals, grain sorghums, sugar beets and various types of beans. He has continued ever since this time to investigate new or improved insecticides that might be used in combination with fungicide for treating seeds of various crops and has compiled a tremendous amount of information on this subject.

UC CONTINUOUS SPRAY SEED TREATER DESIGNED BY ROBERT A. KEPNER, 1948

About this time, I became interested in the methods of treating seeds because when applying the fungicides as a dust, it did not adhere well to the seed and was very offensive to the operators who were treating the seeds. I became quite interested in the possibility of treating the seeds by means of a spray containing fungicides either in solution or dilute suspension. Dr. [G. F.] Miles of the DuPont Company was interested in the same general area. He developed what he called a slurry treater in which the wettable fungicide was mixed into a very thin slurry about the consistency of buttermilk and this was smeared on the seeds as they passed through the slurry treater. This proved to be quite effective with smooth seeds such as beans, peas, wheat and corn, but with the rough absorbant seeds as we have with sugar beet, it did not appear to give a uniform distribution of fungicide and, in my opinion, it was possible to do a better job by applying the material as a spray.

I worked with a number of plans in an attempt to develop a system where the spray could be applied uniformly to the seed, where the unit would be self-cleaning and have the capacity to treat the seed at a fairly high rate per hour. I finally developed a plan that seemed to me would be a possibility. I drew a sketch on a sheet of yellow paper and took it to Roy Bainer in ag engineering and asked him if he thought that we could make this type of a spray treater work. He called in Robert [A.] Kepner who was quite interested in the project, and so Kepner and I underook the development of a model for a spray seed treater.

What was the first step?

After a number of trials, Bob Kepner came up with a model which consisted of a horizontal cylinder about three feet in diameter and six feet long. This cylinder was lined with three panels of corrugated aluminum siding,which provided a lining that picked up the seed at the bottom of the rotating cylinder and carried it up about ninety degrees. Then the seed would fall down the face of the ascending layer of seed. The cylinder was driven at a low rotation speed and the seed was fed in from one end. The spray was introduced by a nozzle mounted opposite the seed layer and sprayed a band about three feet long and about one foot wide across the face of the ascending and descending layer of seeds. In this way, with a minus two degree slope, the seed moved forward about three or four inches each rotation. This meant that each seed passed through the spray zone about eight times in moving through the three foot zone of spray. Then the additional three feet provided for after-mixing of the seed. By using a suspension of about four percent of the weight of the seed, it was possible to get a very uniform coverage of the suspended fungicide or insecticide and still, with sugar beet seed, did not provide an excess of moisture on the seed.

In my opinion, Bob Kepner did an excellent job of engineering on this sprayer. Eventually, we came out with a completed unit which became known as the UC Continuous Spray Seed Treater. The University considered patenting this seed treater, but the decision was that the demand would probably not be great enough to justify the expense.

Well, how did you get any of these things produced for commercial use and when did this occur?

In 1949 after we had completed all of our preliminary trials, we showed it to the Blackwelder Manufacturing Company of Rio Vista, and they agreed to build a number of custom built units for the sugar companies that would place orders. All together, about six or eight of the units were built and this was enough to treat practically all of the sugar beet seed in California and in most of the other Western states. Most of these units are still in use.

SEED TREATMENTS FOR OTHER CROPS

Well, Lysle, we've been talking about damping-off problems of sugar beet seedlings caused by various fungi and you have gone through quite a bit of the work that you did with regard to seed treatments and how this led into the development of processing equipment for treating the seeds and so on. But I'm quite sure that over this time period there must have been some other kinds of research that you were involved in that had to do with the better understanding of these particular diseases. Can you tell us a little bit about some of those kinds of research?

Up to now I've talked primarily about seed treatment with sugar beet seed. But during the same period, I also worked with seed treatment on a number of other crops. And working with these, it seemed clearly evident that the severity of seedling disease was strongly influenced by environmental conditions. Soil temperature and soil moisture are two of the most important limiting factors. Some pathogens cause disease only at low temperatures, while others are destructive only at moderate or high temperatures. In the same way, there are some crops such as cotton or watermelons that are seriously affected by seedling diseases in cool soil but which escape infections at high temperatures. In contrast, crops such as spinach and sugar beet are often more seriously attacked at moderate or high temperatures than in cool soils, providing soil moisture conditions are similar.

Many other investigators have conducted experiments to determine whether the primary effect of temperature was upon the pathogen or upon the host, with reference to seedling disease. After observing a large number of tests conducted in constant temperature chambers, with uniform soil moisture but involving different combinations of host and pathogens, I concluded that the effect could not be assigned primarily either to the host or to the pathogen, but represented the interrelationship of the two.

In searching for a single measure representing this relation, I found that the ratio of growth rate of the host and the growth rate of the pathogen at different temperatures were inversely related to the severity of infection. This theory, in effect, suggests that with all of the other factors constant, if the temperature is relatively more favorable to the host than to the pathogen, the degree of infection will be minimized. And conversely, if the temperature is relatively more favorable to the pathogen than for the host, the disease will probably be severe. The test of this hypothesis is to use some measure of growth rates that can be compared in a number of combinations.

How did you initiate this research?

In our studies, I first undertook to determine the rate of emergence of seedlings of a number of crops. This was measured by the coefficient of velocity of emergence, a term that was coined by Dr. [F.] Katowsky, a Polish scientist, when he was working at Davis in 1925. He was a professor on leave from the University of Warsaw. Next I determined the growth rate of a number of pathogens causing seedling diseases by growing them on solid agar, or in nutrient solutions. Then I plotted the two growth curves and determined the ratio of the growth rate of the host compared to the growth rate of the pathogen. Next I planted seeds of various crops in soil infested by a specific organism and measured the percentage of emergence at different temperatures. With this information in hand, I could compare the percent emergence of a specific crop grown in soil infested with a specific pathogen and relate it to the ratio of the growth rates of the host and the pathogen over the full range of temperatures. The results showed that with a crop like spinach, known as a low temperature crop, growing in soil infested by Rhizoctonia which grows at moderate to high temperatures, the spinach seedlings would usually escape infection at low temperatures. Infection would increase in severity with increasing temperature and was most severe at the highest temperatures at which spinach will germinate.

In contrast, a crop like cotton which does not germinate below about fifteen degrees Centigrade (fifty-nine degrees Fahrenheit), grown in soil severely infested by Pythium which grows relatively well at low temperatures, we found that the cotton seedlings would be rotted before emergence, or killed by Pythium soon after emergence at the lower temperatures. But as we increased the temperature toward thirty degrees Centigrade, or eighty-six degrees Fahrenheit, we reached a point where little or no infection occurred at the higher temperatures.

I guess this means that you certainly don't want to plant cotton in cold soils and expect to be free of seedling pathogens.

This is true and cotton growers had learned this long ago. They will test the temperature of the soil and if it is below about sixty degrees, they will delay planting until the temperature is above this point for three or four days in a row.

Now applying the same concept to sugar beet, which is a relatively low temperature crop, if we take an organism like Aphanomyces or Rhizoctonia, which does not grow well at low temperatures but grows relatively better at high temperatures, we find that by planting sugar beets in February or March, we can escape infection by Aphanomyces or Rhizoctonia. But in contrast, if the seed is infested by Phoma, a low temperature organism, the most severe infection occurs at the low temperature and it becomes much less as the temperature is raised toward the optimum for sugar beet germination. However, the growth rates of sugar beet and the fungus Pythium are so similar over the whole temperature range that it is nearly impossible to escape infection by Pythium at either low or high temperatures.

The conclusion, therefore, is that you cannot escape the infection of Pythium by modifying your planting dates. Rather, it is essential to have good protection by either seed treatment or soil treatment over the full temperature range. This concept explains why, in general, the high temperature crops such as cotton, cow peas, lima beans, and peanuts are more subject to seed decay, or pre-emergence damping off at low than at high temperatures, whereas low temperature crops such as spinach and peas often suffer less infection at low temperatures than at intermediate or high temperatures. This, of course, is provided that the soil moisture conditions are similar. If this relation has general applications, then one can establish a temperature range within which infection is absent, a range within which infection is moderate, and one within which infection is likely to be severe for any combination of host and pathogen for which the growth rates of the two are known. By studying seasonal temperatures, one is sometimes able to plant certain crops within escape periods for specific pathogens, or at least to avoid the period of most severe infection. If plantings of susceptible crops must be made during the period favorable for infection, then seed or soil treatment with a fungicide is imperative, and one should select the specific seed or soil treatment most effective against the organisms most likely to operate at a given soil temperature.

A LANDMARK PUBLICATION, 1947

Lysle, I think this is a good place to point out that you published this research in a paper in the Journal of Agricultural Research *in 1947. The title of this paper was "Growth Rates of Host and Pathogen as Factors Determining the Severity of Pre-emergent Damping Off," and that this paper has more or less become a phytopathological classic, and that many people still refer to this work in terms of getting a better understanding of many of these organisms that attack seedlings.*

It is true that this paper attracted a good deal of attention and we have had many requests for reprints over the years.

Now, you pointed out that there are some situations where seed treatments just aren't adequate to control these damping off fungi. What research were you involved in to attempt to do something about these more severe situations where seed treatments wouldn't work too well?

During the 1940s we recognized that seed treatments could not be depended upon to solve all of our seedling disease problems. So we often talked about the possibility of identifying some of the mild fungicides that might be introduced into the seed row at a somewhat higher dosage than used for seed treatment and in that way we might develop fungistatic conditions which would protect the seedlings against this type of infection.

Our first trials were made in 1946 in the Salinas Valley in cooperation with Dr. Snyder of Berkeley. We were trying to protect bean seedlings from diseases caused by Rhizoctonia and Fusarium. With the materials we were using at that time, we were only partially successful but we did gain some valuable experience. A few years later, we decided to use fungicides on brussel sprouts, applying the fungicide in the transplant solution as they were transplanting the brussel sprout seedlings from the plant beds into the commercial fields. In the Half Moon Bay area these plants frequently became infected by a disease known as club root and we tested several fungicides in the transplant solution to see if we could prevent this type of infection. The most effective material we identified at that time was PCNB, the same fungicide that we had used as a seed treatment for control of Rhizoctonia with sugar beets. This treatment is still in commercial use twenty-five years later.

Soon thereafter, we were asked for help by the growers of garlic, a crop grown at that time primarily in a concentrated area from Salinas to Hollister and Gilroy. We tried several chemicals and PCNB appeared to be the best for the control of a rotting disease called white rot. Here we had to use as much as twenty pounds per acre of the dust applied in the furrow as the cloves of garlic were planted by machines. With this treatment we could get eighty or ninety percent control of the white rot over the whole period of growth and maturity of the garlic. Since this is a very expensive crop, the growers found they could readily afford this type of localized control.

Well, we talked about seed treatments and chemical row treatments. Were there any other developments in relation to your studies on seedling diseases that we should talk about?

COTTON RESEARCH WITH RICHARD GARBER, SHAFTER FIELD STATION

I worked for several years with Dr. [Richard H.] Garber on control of cotton seedling diseases. At first Dr. Garber was located at Davis and we conducted a number of trials in which soils from cotton fields were brought into the greenhouse and tested for the severity of seedling diseases and the organisms that were involved. We then tried specific combinations of fungicides for the control.

Lysle, Dick Garber is a USDA scientist. He was first stationed at Davis and is now stationed at the Shafter Field Station.

That's right. He was stationed at Davis during the period when he was working for his Ph.D., and upon completion, he was transferred to the cotton research station near Shafter.

We continued our cooperative work, but we made a division of effort. I was to continue the trials in the greenhouse, working particularly with new fungicides. I would determine the effectiveness of the fungicides as seed treatments or soil treatments, and then determine the range of dosage that would be free of toxicity to the germinating cotton. Then Dr. Garber would take our best results and put them out into field trials throughout the cotton area in the San Joaquin Valley. In that way we were able to cover the range from the availability of a new fungicide to its evaluation in the greenhouse and in cotton fields.

What seedling pathogens were found in cotton fields?

In proceeding with the investigation, we found that the cotton seedling disease found in nearly all fields was caused by Pythium, but that in many fields Rhizoctonia infection of seedlings also occurred. At that time mercury seed treatments were commonly used on cotton seed. This controlled Pythium infection, but was not effective with severe Rhizoctonia infestation. We therefore combined PCNB with the mercury treatment to provide better control of both organisms. Later when it became illegal to use mercury seed treatments, we used Captan, Terrazole or Dexon in combination with PCNB for the same purpose.

These combination treatments seemed to have solved a number of the seedling diseases you worked with.

In some cotton fields the Rhizoctonia infestation was so severe that seed treatments alone were not satisfactory, so we tested the introduction of fungicides directly into the seed furrow as a spray or dust. At first we used a spray consisting of dithane reacted with zinc sulphate. This proved to be effective and was widely used for a few years, but was later replaced with PCNB because we found that it could be dusted into the seed furrow with much simpler equipment. Some of the growers found they could add a measured amount of a ten percent PCNB dust directly to the seed hopper and as the seed was planted, the PCNB dust sifted through the seed mass and effectively dusted the surface of the seed furrow.

An interesting secondary benefit came out of our early trials with dithane reacted with zinc sulphate and sprayed into cotton seed furrows. A young man by the name of Audie Bell was working at the Shafter Station and observed our trials. Later he was employed by the J. G. Boswell Company to conduct trials with fertilizers and pesticides on their huge (90,000 acre) Tulare Lake ranch near Corcoran. He conducted trials with the dithane spray and observed that the cotton seedlings in treated rows were much more vigorous than in untreated rows, even where seedling disease was not severe enough to account for the difference. He concluded that the zinc sulphate included in the spray corrected a severe zinc deficiency. In large scale trials he found that thirty cents worth of zinc sulphate per acre increased the yield of cotton by one and a quarter bales (about 600 pounds) per acre.

Dr. Garber is still at Shafter and is continuing his work with seedling diseases, now in cooperation with Dr. James DeVay, chairman of the Davis plant pathology department.

Well, it's very interesting to me to see how much of this work all the way from rather basic studies that you've done was always aimed at controlling these practical problems and was accomplished by cooperation with many other individuals. Particularly significant is that all of these things that you've found out have been used in actual commercial production and have had a big effect on several industries. Were there any other developments with regard to seed treatments that you have found interesting?

F. J. HILLS: GRADUATE RESEARCH ON SYSTEMIC FUNGICIDES

I would like to refer back to our discussion of our early trials with Dexon as a seed treatment and, Jack, I'd like to mention that your research with this fungicide contributed valuable information leading to its more effective usage. I would like you to discuss briefly *your* work concerning systemic action of the fungicide and other information relating to the chemical decomposition of the material.

Well, okay, but I'll only do this from the standpoint of my being a graduate student, coming back to school and starting to work on a Ph.D. problem. I really did not have much in the way of ideas as to what to work on, but was lucky enough to have a major professor, you, who was interested in many things and who always had many problems that needed further attention. Your guidance of me and other graduate students not only helped us get through but always contributed a little bit towards understanding a disease problem. In my case, it was a better understanding of how Dexon did its job in controlling seedling diseases of sugar beets.

I think the most interesting thing about Dexon is that you had already shown how effective it was as a seed treatment and were interested in learning just what its mode of action was. Why was it so much better than many other materials you had tested? And so we set about to test the effect of this material on the growth of the fungi involved, particularly Pythium and Aphanomyces, both in pure culture techniques in the laboratory and for controlling disease in infested soils in the greenhouse. One very interesting thing we found I'll never forget.

One day I was doing some work in the greenhouse and I had a flask of Dexon dissolved in water which was a bright yellow color in solution. I was applying this solution to soil in the greenhouse to see how well it would control damping off caused by Pythium when introduced into the soil. It came lunchtime and even graduate students have to eat sometime, so I left the greenhouse for about an hour. When I came back, I looked around for my yellow flask and couldn't find it anywhere. There was just a flask of clear water sitting on the bench. It finally dawned on me that this was the flask I was looking for and that the yellow color had all of a sudden disappeared.

Well, this right away left open some interesting possibilities for additional research, and just shows how sometimes a little bit of luck can help a student along the way. So subsequently, we investigated the effects of Dexon in its yellow state versus its effects after it changed to a colorless compound and found out that as a colorless compound, it was ineffective. It also had no effect on the growth of the Pythium pathogen in shake culture in the laboratory and it had no effect as a seed treatment or as a soil row treatment. So we learned one interesting thing, that you had to be careful of how you handled that very useful material if you're going to make it effective as a commercial treatment.

Another thing that we did, very briefly, was that we wanted to see whether or not the hypothesis was true that this fungicide may have some systemic action. That is, it's taken up to some extent through the roots of the plant to make the hypocotyls of seedlings more resistant to infection. After many different kinds of studies, I think I was able to show that this indeed was true. This material was systemic and that probably a good deal of its effectiveness was due to the fact that this was true. It was systemic and was protecting hypocotyls, whereas most non-systemic fungicides wouldn't have this property. So very briefly, that's the story of Dexon and my getting a Ph.D. in plant pathology.

I think the information that this fungicide was systemic tended to explain why it was so effective when it was applied to soil in contact with the roots of the plant but not necessarily located in the area where infection occurred. With row treatments where Dexon is used, or with seed treatments when infection occurred at some distance from the seed, I think this is an essential part of the effectiveness of this material.

Now you've spoken about the loss of color from your yellow solution. If you left these dilute solutions of Dexon for longer periods of time, what happened to the color?

Well, actually, it gradually changed from a colorless solution to a very pale purple and finally to a very dark purple, depending upon the concentration of Dexon. We have tested both the colorless and the purple forms against Pythium and found that only the yellow form was effective in controlling the disease, while neither the colorless nor the purple form had any effect.

Later in the discussion of some experiences, I'll probably refer back to this question of decomposition, but I think this is sufficient at this time.

XVI

OVERSEAS ASSIGNMENTS IN POSTWAR ERA

Another thing that I remember over this time period [after World War II], was that you were becoming so famous with regard to seed treatments and seed processing that a number of people would visit Davis and see your work. And this led to some foreign assignments to help people in other countries work on some of their sugar beet problems. Can you tell us a little about some of these.

I'm not sure it's the result of being famous, perhaps a matter of a little luck in being at the right place at the right time. But you are correct in saying that I have had some very interesting foreign assignments, and I'd like to discuss some aspects of these.

First, in about 1950 we had a visitor from Denmark whose name was Professor Thomas Rosenstand. He was the owner and the director of an agricultural school near the small town of Korinth, on the island of Fyn. In 1950 he was sent by the Danish government to find out everything that he could about the processing of sugar beet seed. They had heard of the work that was being done by Roy Bainer and Mr. Rosenstand came here and also visited other locations to collect information. I've mentioned earlier that I spent a good deal of time during the forties working with Roy Bainer on the question of the most effective treatments for the processed seed that he developed. While Roy was in England in 1945, I operated the equipment and tested the effect of processing on the different samples that were developed. I also made several field plantings and evaluated the results of seeds processed in different ways. So I'd had a good deal of experience with the decorticator and in the mechanical aspects, much more than you would ordinarily expect for a plant pathologist. When Mr. Rosenstand came to Davis, Roy Bainer was busy with some other projects so I took him through the laboratory where we were processing seed, explained what we were trying to accomplish, showed him the equipment and how it operated, and we then visited a number of fields where we could talk with growers about the results and the amount of labor-saving that resulted from the processed seed planted with precision seeders.

Mr. Rosenstand was very interested in this work and asked if it would be possible for me to come to Denmark under the Marshall Plan which had been recently developed to aid economic recovery in Europe after World War II. I said that I would be very interested in doing this if the proper arrangements could be made, but that I would need to know some time ahead so I could make other arrangements for the research work and the teaching then in progress. He returned to Denmark and I didn't hear anything for several months.

INVITATION TO DENMARK, 1951

But eventually, in the spring of 1951, I received an invitation through the minister of agriculture in Denmark indicating that they had applied for a project under the ECA [Economic Cooperation Administration], which administered the Marshall Plan. He asked if I would come to Denmark sometime during 1951 and spend approximately two months working with them on seed processing, seed treatments, and other problems related to sugar beet production.

About the same time, I was contacted by people in Ireland who indicated that they had followed our seed treatment work, and had tried it with the seed used in Ireland, but had had very little success because of the severity of Phoma infection on their seed. I made it clear to them that Phoma was not a particularly severe problem with us because most of our seed at that time was being grown in the arid Southwest where very little rain fell on the seed during the period of maturing or harvesting of the seed crop. They pointed out that in Ireland they have an abundance of rain and that you could expect some rain during the whole time they were maturing and harvesting their crops. Because Phoma infection was so severe, they had not been able to make any progress toward precision seeding or mechanical operation in the beet crop. The Phoma was the limiting factor in securing a stand and they needed a way of curing this disease before they would be able to modernize their sugar beet farming. They also suggested that I spend some time in Ireland, but I told them that I had already made a partial commitment to Denmark and I didn't know if it would be possible to do both unless they could be combined in some way.

It developed that I heard essentially nothing from the ECA in Washington until late in the summer. Jane and I gave up any thought that this appointment would be approved, so we took a week off and went on a pack trip for fishing in the mountains. On our return, we found that they had been calling urgently from Washington to report that the project *had* been approved and they wanted us to leave almost immediately to fly back to Washington and from there to Copenhagen to carry out the project that Mr. Rosenstand had proposed. It wasn't quite that simple to make all the necessary arrangements, but we did manage to do it within the next couple of weeks.

Did you take any of your children with you?

No, not on this trip. Joann had just graduated from UC Berkeley and was working in San Francisco. Douglas was in the U.S. Army, stationed in Taiwan, and Janet was in the first grade.

We drove from Davis to Ames, Iowa, where we left Janet and Jane's mother [Margaret C. Rhoads] with Jane's sister, Betty Lee.

I flew into Washington, D.C., for an indoctrination in which they told me how they would expect us to conduct ourselves in the foreign country and the subjects that we must avoid.

Were you briefed by State Department officials?

Yes. I remember particularly that we were not to drink to excess at social gatherings and we were not to make undiplomatic statements in the presence of the people from the host country. We felt that we could handle this fairly well, so we were somewhat amused at the extent that they went to to explain these things to us. But I assume that they had had some bad experiences in other cases and were always concerned that this might happen again.

Had either you or Jane traveled overseas before?

No. This was our first trip abroad. Anyway, Jane and I finally met in New York and I flew on SAS from New York to Copenhagen. Jane was not very anxious to fly across the Atlantic and decided to take the Queen Mary across to London, and then flew to Copenhagen. I flew directly from New York to Copenhagen, with stops at Gander and at Preswick in Scotland. Upon my arrival in Copenhagen [in September 1951], I learned that through an error in the arrangements in Washington, a considerable group had gathered the day before to meet me and to hold the first planning meeting for the trip. Unfortunately I arrived a day later than they had expected, which was a little embarrassing.

The first week they sent me to Maribo on the island of Lolland, where one of the Danish sugar factories was located. Here they had a decorticator purchased from the Blackwelder Mfg. Co. [of Rio Vista, California]. They had been attempting to use it, but were somewhat disappointed in the fact that it did not produce single germ seed. They had read a good deal about segmented seed and were not clear on the difference between the segmenting and the decortication process. It took me some time to explain to the gentleman at the sugar factory and to others in Denmark that decortication was quite a different process from segmenting and that the main purpose was to produce a seed unit which could be readily planted by a precision seeder, equally spaced in the row so we could minimize the amount of labor required for the singling process. I explained that decorticated seed could be produced with about fifty percent doubles and fifty percent singles, with high percentage of germination and good planting characteristics, but we could not produce units with as high a percentage of singles as was done with the segmenting process.

Well, actually Lysle, fifty percent doubles as far as the seed is concerned would probably not equate to fifty percent doubles in the row because in many of those seed pieces with two seeds, one seed wouldn't come, isn't that right?

That is correct, particularly when the emergence in the field represents not more than thirty to sixty percent of the potential emergence of that seed. This was a concept, however, that was rather difficult for many people to accept because up to this point their focus had been on the idea of getting as near as possible to a single germ planting unit. We felt that we were accomplishing the labor saving that was the ultimate purpose with a more satisfactory type of seed from the standpoint of seeding and emergence of the seedlings where we really wanted them.

I think, too, work around that time and probably since that time has shown that maybe twenty-five to fifty percent double seedlings, so long as those plants are spaced fairly far apart—say six to eight inches—would have little or no effect on the final beet yield. If we hadn't had the development of genetic monogerm seed, I'm quite convinced that we'd be using decorticated seed today just about as successfully as we are using monogerm seed.

I agree that we could have continued for several years with decorticated seed, but the industry seemed very anxious to shift to monogerm seed as soon as suitable varieties with this character were available.

Are there any other things that you found particularly interesting about this trip to Denmark?

After the first week at Maribo, I returned to Copenhagen and met Jane who had arrived just the day before. Mr. Rosenstand then took us on an extensive trip through a number of the islands that make up the small country of Denmark, and finally to Jutland which is the peninsula extending north from Germany and is the only part of Denmark connected by land to northern Europe. We visited some twelve locations where decorticated seed had been planted in comparison with plantings of whole seed, screened to a small size. The decorticated plantings were somewhat disappointing because of reduced emergence. They had sent seed to California to be decorticated and returned for field plantings, but the seed arrived so late that we did not have an opportunity to make preliminary trials. Instead, we had to run the seed through the decorticator at a setting comparable to that used in California. It turned out that this seed was somewhat softer in texture and was more severely reduced by our decorticator than was true with most of the United States grown seed. Therefore, we reduced the germination but by careful screening, we were able to recover enough seed for their field trials. In general, these trials indicated that satisfactory stands were secured with the decorticated seed and that there was a saving in labor from ten to twenty-five percent over the light seeding rates with whole seed plantings.

I should digress to say that sugar beets are a very important crop in Denmark, both because they represent cash income from the production of sugar and because all of the tops and the beet pulp are used for feeding of livestock. There were, at that time, about 175,000 acres of sugar beets grown for sugar in Denmark, which was less than in California. In addition, however, they grew about three times that much of fodder beets ranging from mangles to specialized crosses between sugar beets and mangles that are used exclusively for livestock feeding.

One of the reasons for the heavy emphasis on sugar beet and fodder beet for livestock feed is that at that latitude they can't grow crops with any great success like we can grow here for forage, such as alfalfa. The fodder beet and sugar beet are probably their most productive crops.

Well, this is certainly part of it. They are about fifty-six degrees north and there's no possibility of growing field corn to maturity, that is, for grain production. They can grow it for silage, but not for grain. They have a relatively short growing season and crops like sugar beets and fodder beets are among the most productive in terms of feed units. And the sugar, as I mentioned, is very important as a cash crop.

Lysle, one other thing that might be interesting if you can remember much about it would be that since it was in late 1951 when you made this trip to Denmark, not too many years after the ending of World War II, you must have been talking with many of the people there that were actively engaged in farming and had been all through the wartime situation in that country. Can you remember something of their attitude towards Americans or about the whole process of World War II and what this had meant to them.

Yes. This was only a few years after the ending of World War II and Denmark had been occupied by the Germans from about 1941 through mid-1945. During this period Germany had plans to use Denmark to produce the food that would support their armies. The Danes were forced to provide a great deal of food, but they also used every means at their disposal to minimize the amount of food that they made available to the Germans. During the first year or so, apparently the Germans treated them quite well, because they wanted to continue to receive as much food as possible from them. But as the resistance movement in Denmark began to exert their efforts, the Germans became very rough. There were many stories about how the resistance movement had interfered with the movement of German troops that were headed for the Normandy front and how the Danes had attempted in every way to interfere with the communications and with the plans that the Germans were trying to carry out. So before the war was over, conditions became very severe because the Germans attempted to punish everyone who did not cooperate or who did not provide as much food as they felt should have been provided. I talked with a number of people who were very active in leadership capacities in the resistance movement. In fact, I have a small porcupine pin that was the badge of the resistance movement during the war.

This is sort of a secret badge?

Yes, they wore it so that others would recognize them, but the idea was that only the people who were in the resistance movement were to know what this badge represented.

It would have been pretty drastic if they'd ever found out the meaning of the badge.

When we returned to Copenhagen, I spent about two weeks at F.D.B., a Danish cooperative seed company, which had a large seed processing plant at Glostrup about fifteen miles west of Copenhagen. They had also purchased a decorticator from the Blackwelder Company and it arrived about the time that I finished my tours in the western part of Denmark. They wanted me to help them set up this machine and to adjust it for use with fodder beet seed. So the first day

I went out on the train, I stopped at a shop and bought a set of coveralls so I could work with the machinery. When I arrived there the decorticator was set up, ready for the mounting of the neoprene pad and the adjustment. I had previously sent instructions of just how to proceed to set this up for use. This involved lowering the neoprene pad so that it rested firmly and evenly on the stone and then raising the pad just enough to produce the proper size of seed unit. Unfortunately, with this particular unit, Blackwelders had built the housing that went around the stone and neoprene pad at such a height that it was impossible to lower the neoprene pad firmly onto the stone, so I was stuck with a list of directions that could not be followed. This had all been translated into Danish and the workmen were prepared to go about it under my instruction.

You were pressed into service as an agricultural engineer, it seems.

Yes, I had to quickly develop another plan which consisted of having them cut four plates of metal exactly four millimeters in thickness. I took these four plates and placed them on top of the stone and then mounted the neoprene pad on top of the metal plates and set the nuts on the bolts firmly at the lower side of the neoprene pad. Then with the nuts in position, I could lift the neoprene pad, remove the metal plates, and then replace the neoprene pad on the nuts in such a way that we had exactly four millimeters of clearance between the stone and the neoprene pad. This was essential as a starting point.

Then we took different lots of seed and passed them through the decorticator at different distances of setting between the stone and the pad, and by screening the material and running germination tests, we were able to determine the optimum setting of clearance between the neoprene pad and the revolving stone. And then we were ready to decorticate large quantities of seed that could be used for planting or testing. In a few days, we completed all the necessary studies and the machine was set up ready for the commercial operation. Of course, it required a great deal of trial and error over a period of time in order to get exactly the amount of seed reduction that was optimum for the particular kind of seed they were using and for the seeders that were to be used.

Well, they finally were able to make it work, though, and had good results with the seed.

Yes. I left there on the tenth of November so most of their commercial work was done after I was gone. But they informed me that it worked out quite well and that they used this process for a number of years, until monogerm seed eventually became available.

Well, knowing the federal government, I'm sure they didn't let you do all this work under a program of this sort without having something in writing to show for it, so what happened in that regard?

Well, during the latter part of my stay in Copenhagen, I was writing nearly day and night to put this all into a report that could be translated into Danish and distributed to the people who were interested. The report was turned in just before I left Copenhagen, and eventually was published by the Royal Agricultural College.

Also during this period, I was asked to give a lecture at the Royal Agricultural College. People from the sugar industry, the fodder beet seed industry and many of the large farmers came into Copenhagen to attend this lecture.

That must have been a busy time for you. What was next?

When we were near the end of our stay in Denmark, I received a phone call from Ireland saying that it had not been possible to finalize a project under the ECA, but they were very

EXPERT IN THE USE OF DECORTICATED SEED FROM THE UNITED STATES COMING TO FUNEN TO ASSIST DANISH SUGAR BEET GROWERS

An attempt is being made to extend the use of decorticated seed in the growing of sugar beets. With the assistance of Marshall Office in Copenhagen, Director Rosenstand of the Agricultural School of Korinth has taken the initiative to invite Professor Leach who is an expert in the field of decorticated seed to come to Denmark to give advice on the correct production of this particular seed.

Decorticated seed has already been used in Denmark to some extent, but the results have not been too good, whereas decorticated seed is used almost exclusively by American sugar beet growers. The Danish decorticated seed is produced at Maribo Sugar Factory where the Danish Sugar Factories have installed a special decortication machine, which was imported from the United States.

Next week Dr. Leach will visit the sugar factory; from there he proceeds to Funen on September 2nd, and later to Zealand and Jutland. On Funen he is going to visit the Korinth Agricultural School and also study the growing of sugar beets at various farms on Funen. He is particularly interested in a study of the developments in mechanization.

Translation of an article appearing in
Fyns Tidende on September 20, 1951.

anxious to have me come to Ireland before I returned to America so they could talk with me about their special problems. So in November Jane and I took a ship from the west of Jutland across the North Sea to Harwich in England. We took a train into London for a short visit and on to Liverpool by train. From there we went by overnight boat to Dublin.

WORK WITH THE IRISH SUGAR COMPANY, 1951

We were met by General [Michael A.] Costello who was a former Lieutenant General in the Army of the Irish Republic, but was then retired and serving as the general manager of the Irish Sugar Company. When we arrived, General Costello took us to one of the finest hotels in Ireland, the Gresham, and located us there for our stay in Ireland.

When he checked us in at the desk, it appeared that he had reserved a suite for us, but the suite was not available until later in the morning because it was occupied by Van Johnson. He was not yet ready to leave. We were glad to settle for a somewhat lesser accommodation, a combination bedroom and sitting room with very luxurious furnishings. We were very comfortable at the hotel and pleased with the excellent food.

Were all your expenses provided for?

Yes, by the Irish Sugar Company. It developed that the sugar company had filed a request with the ECA for an assignment for us to remain in Dublin for approximately two months and since we were already there, it only took some rapid communications between Dublin and Washington and the project was approved. So we were back on the Marshall Plan within forty-eight hours after our arrival in Dublin.

What were the critical sugar beet problems in Ireland at that time?

They pointed out that their problem of Phoma on the beet seed was really quite critical because with the seed produced under wet conditions in Ireland, there was a very heavy infestation of Phoma which very often penetrated deep into the seed ball in close association with the embryo. None of the seed treatments they had used had given satisfactory control. They had heard that I had worked with seed treatments for Phoma and wanted to know if I had any suggestions that would improve their condition.

Well, it turned out that sometime prior to this the growing of sugar beet seed in the western United States had shifted largely from southern California and Arizona to the Pacific Northwest. Most of the production was centered in the Willamette Valley of Oregon, with a smaller amount in the Medford area. Particularly in the Willamette Valley, they had a considerable amount of rain during the early spring and occasional showers during July and August, the period of maturity and harvesting of the seed. In addition to that, the land was quite rolling and the only way they could irrigate during drier periods was by the use of overhead sprinklers. So between the rainfall that occurred quite frequently and the overhead sprinklers, there was an abundant opportunity for Phoma to develop upon the leaves and seed stalks and later directly on the seed. In some years there would be a very high percentage of Phoma on the beet seed balls, but usually, by the time the seed was processed and treated with our ordinary protective fungicides, we would not have excessive amounts of seedling infection when this type of seed was planted in California, especially if it was planted late in the spring when the temperatures were moderate, or higher.

So by the time you were in Ireland, you had had some experience with some of these deep-seated Phoma infections and had some idea of how they might be treated to control them.

Yes. From my work at Davis with Phoma infected seed, I decided it would be desirable to have a treatment that would completely eliminate Phoma infection from the seed stocks that were to be increased in new areas. I tried a number of different treatments and finally settled on one that was extremely effective. I found that if I dipped the seed into a solution of ethyl mercury phosphate, diluted one to twenty-four thousand, or approximately forty parts per million, for a period of twenty minutes, the fungicide would penetrate deeply enough into the seed to kill most of the mycelium and spores and would leave a residue of the ethyl mercury phosphate that would serve as further protectant against any of the other seedling diseases that occurred. In a trial with seed from different areas in Oregon, with this method called the EMP steep, I was able to completely eliminate Phoma from twenty-eight of thirty-two seed lots, and the others had a very low amount of infection. This was a rather difficult seed treatment procedure, because it required steeping the seed for twenty minutes and then drying it before it could be rebagged for storage. With the type of seed that was produced in Ireland, however, such a procedure appeared warranted.

So they took me to the Albert Agricultural College in the north of Dublin and here I met Professor Robert McKay, who was the plant pathologist with the National University. He made arrangements for me to use part of his laboratory and part of his very limited greenhouse facilities. Upon my request, they furnished me a number of seed lots grown in different parts of Ireland and with different amounts of Phoma infection. They also procured a collection of soils from several of the counties in southern Ireland so that I could make a further determination of what seedling diseases might be coming from the soil. Then I settled down to conduct trials with their seed that was treated in different ways, including my ethyl mercury phosphate steep, also checking against the organisms that were soil-borne.

After several weeks of investigation, it became quite clear that the Phoma infection on Irish grown seed was so severe and so deeply embedded in the seed balls that ordinary dusts or superficial seed treatments had little chance of success. In fact, the ethyl mercury phosphate steep was not able to completely eliminate infection. However, I did succeed in about a ninety-five percent control of the seed-borne Phoma infection with their worst seed lots and this was far better than they had been able to do with any other procedure.

At the the end of these trials, I reported to the Irish Sugar Company at a meeting and discussed the various observations and investigations that I had been able to make in this short period of about six weeks. Since the EMP steep seemed to be the most effective measure that I'd been able to identify, I suggested that they treat perhaps a hundred pounds of seed and make plantings in the following spring in different areas of southern Ireland where sugar beets were commonly grown. They could then judge from the field plantings whether the treatment by the ethyl mercury phosphate steep was enough better than the other treatments they'd been using that it would be worth considering as a commercial procedure in the future.

So we left Ireland in late December and went to England for New Year's with Dr. Raymond Hull and his family. Jane and I came home together aboard the *Isle de France,* checked in at Washington, D.C., and eventually we got back to Davis in late January 1952. A few weeks later, I had a letter from General Costello saying that they had been evaluating my report and the trials that I had conducted. He said they were so favorably impressed with the results that they had decided to set up the equipment and to treat *all* of the seed for planting in Ireland that spring.

They didn't even bother with any field tests?

This was the thing that startled me, because this treatment was an experimental procedure.

It had never been used in the United States, except that I had made a few trial plantings in Oregon. Otherwise, it was really a laboratory or an experimental procedure.

You must have held your breath for quite a long while, waiting for the Irish seed to come up.

That's right! I knew that if this failed that I probably would never be going back to Ireland. I was very concerned that they would use the EMP steep on such a grand scale, without any field trials. In fact, they treated nearly a million pounds of seeds and planted over fifty thousand acres of beets with this treatment, which had never been tested in the field.

And what happened?

Well, fortunately, the crop came up and the reports were that Phoma infection was very much less from seed that had been treated with the ethyl mercury phosphate steep and that in their opinion the whole procedure was a great success. During the years following 1951, Professor McKay and others followed the trials in the field and they published reports indicating that for the first time, they had successful control of Phoma on their Irish seed. This was quite significant to me because when I had first arrived at Professor McKay's office, he showed me the manuscript for his new book, "Sugar Beet Diseases in Ireland." I had read in this manuscript that Phoma was so severe on seed produced in Ireland that it could not be controlled by seed treatment. And yet I was there attempting to show how it *could* be done. Professor McKay explained that by seed treatment, he was referring to dusting or surface application of fungicides and did not have in mind a steep treatment of a material as highly fungicidal as ethyl mercury phosphate.

There was one rather amusing incident that grew out of my work in the greenhouses at Albert Agricultural College. The Irish Sugar Company furnished me an assistant who helped with the plantings, took care of the watering, the counting and in general learned all of the procedures. As we approached the Christmas period, he indicated to me one day that he had a real problem because he knew someone had to stay and do the watering and take care of the seedlings in the greenhouse, but that he was engaged to a young lady in the west of Ireland and they were expecting him to come home for Christmas. So did I want him to remain in Dublin, or would it be possible for him to return to his home over the Christmas vacation? Well, I assured him that I was going to remain in Dublin, and really had nothing else to do over the Christmas period. Therefore, I would be glad to do the watering and counting and anything else that needed to be done and that he would be free to go home and take care of his family affairs.

Jane and I drove a car out to the greenhouse on Christmas morning. I finished counting the seedlings and was checking some of them under the microscope when a man suddenly came running up the steps and seemed quite amazed to see us in the laboratory. He apologized and said he thought something must have gone wrong for someone to be there on Christmas morning. We didn't think anything more about it until some time later one of our friends reported to us that at a later meeting Professor McKay had said, "I am not a very religious man, but I am not like Leach, he went to work on Christmas day."

So, from that I have had somewhat of a reputation in Ireland, although perhaps not a very favorable one.

No. I'm sure it's pretty favorable. They probably appreciated very much that somebody would see that that work was done, Christmas or no. I trust you were still able to enjoy some of the holiday festivities. Weren't the Austin Armers from Davis also in Ireland at that time?

Yes. Austin Armer, an agricultural engineer with Spreckels Sugar Company, with his wife and youngest daughter arrived in Ireland a month before we did. They had traveled throughout southern Ireland observing sugar beet harvesting methods but had returned to Dublin and were living at the Gresham Hotel the same time we were.

Soon after we arrived in Ireland, the lady at the desk would ask us whether we were going to be there for Christmas. We merely told her that we were not sure; we hadn't made plans that far ahead. After she asked us several times, we finally concluded that she really wanted to know because it was customary for people from England to come over for the week from Christmas to New Year's and spend the entire week there at the hotel. The rates were doubled and they put on a very festive season for all of those who were staying. What she wanted to know was were we going to give up this room so that she could reserve it for some of the English visitors. Along with the Austin Armers, we decided that we would stay during this period and enjoy all the festivities. Several days before Christmas they began to set up tables and decorations in the entry way to the dining room. These included pheasants and hogs heads and very colorful arrangements of food of several kinds. This we found was being prepared for the Christmas buffet. During Christmas week, they would have the dance in the dining room very late in the evening, starting after dinner usually from ten and it would run until one or two o'clock in the morning.

Every night?

Every night. To participate, we had to have formal clothing. Austin and I had not taken our tuxedos with us so we went to a shop in the southern part of Dublin and rented tuxedos. These were to be picked up on the afternoon preceding Christmas eve. Jane and Alberta went over and picked up the suits and as dinnertime approached, I tried mine on and then found that they had made a mistake in the trousers. They were six inches too big around the waist and about six inches too long. Jane had to do some fast needle work to put it together so that I could even wear them because it was too late to make any exchange.

It's good you married a home economics major.

Well, I looked all right when I was sitting down, but when we got up to dance, the seat of the pants was hanging way too low for any kind of a fit. So it really wasn't a very successful rental procedure.

Were you and Austin Armer both doing work with the Irish Sugar Company?

Yes, he was also on an ECA assignment with the Irish Sugar Company. His job was to study the conditions in Ireland and to develop information concerning the kind of harvesting equipment that would be successful under Irish soil and climatic conditions. He made a very thorough study of this and came up with what I thought was an excellent report of the kind of equipment that would be necessary to be successful there. Austin understood that this would complete his obligation under the contract, so after turning in his report, he and Bert took two weeks for a trip to England, Holland, and France. When Austin returned to Dublin, a second meeting was held where General Costello told him that they had studied his report and were very well satisfied with it. He also said, "We have made arrangements to build such equipment. If you will draw the plans and the list of specifications of the equipment that will be needed, we will arrange to put it together at Carlow before you leave." Austin's reaction to us was that "I didn't come here to invent a new harvesting machine; I merely came to evaluate the situation." In about three weeks, they were able to collect material and put it together so they had a harvesting machine that they could actually bring beets in from the field and run them through the harvester in the shop and test out the topping and loading equipment.

And that was all done in three week's time?

That's right. Later they developed what they called the Armer harvester. This has been modified considerably over the years but still is used almost exclusively in Ireland and it has also been very successful in trials in England and on the Continent. One of the functions of the Irish Sugar Company now is the manufacture of specialized equipment for harvesting of beets.

And it all started from that ECA project?

Yes, that's right.

Your own achievements in Ireland seem to have had considerable impact on sugar beet production.

Well, I mentioned that the treatments seemed to be a success and although it took a great deal of labor to do the treating, and particularly the drying of the seed, they thought it was well worthwhile. People in the sugar beet industry in England were following this work, but were not very interested in the procedure because they felt that it was too cumbersome a method of treating. However, I furnished Dr. Raymond Hull, Director of the Brooms Barn Experiment Station in England, with the exact procedure. He treated their seed and put it into his uniform trials three years in a row. Each year, the seed treated by the EMP steep method showed better seedling emergence and less Phoma infection than any of the other treatments. So after a five-year period, the British Sugar Company also started using the ethyl mercury phosphate steep and has continued to use it on most of the seed treated in England.

The Irish Sugar Company, since 1951, treated all of their seed by this method until in the late 1960s, when they shifted their seed production to Italy and southern France where the climate was drier and warmer.

Since the seed from this area carried much lighter infestation of Phoma, it was then possible for them to discontinue the use of the ethyl mercury phosphate steep and to use protective fungicides.

Well, does that end the story of your association with the Irish sugar beet industry?

No, in fact, I have continued a rather close association with them over a period of time but at rather infrequent intervals.

CONTINUING RELATIONSHIPS IN IRELAND

In 1956, I went to Europe on a sabbatical leave and the Irish Sugar Company invited me to spend a few weeks in Ireland reviewing what they had accomplished and to make suggestions for improving their procedure. While in Dublin, General Costello made an appointment with Ambassador Taft, who was then the U.S. Ambassador in Ireland. General Costello and I spent about an hour with Ambassador Taft discussing particularly their plans for developing an agricultural research institute in Ireland to be financed by the Marshall Plan of the United States. General Costello also discussed the work that I had done in 1951 on the control of seed-borne Phoma, and expressed the opinion that the work that had been accomplished had been worth at least a million dollars a year to the Irish farmer and to the Irish Sugar Company.

They had been using the ethyl mercury phosphate steep that I introduced for a period of five years. They reported that this had been very successful in controlling the Phoma infection and had made it possible for them to go ahead with modern mechanization of their sugar beet production. Apparently General Costello, or someone in the Irish Sugar Company, told Michael Dillon about this. He was the commentator on Radio Erin, the local radio station in Dublin. As a result, he taped an interview with me and later as we travelled through Ireland, we frequently heard people say, "We heard your interview with Michael Dillon on the radio." The *Irish Press* also sent a reporter who interviewed me with reference to the work that had been done and the results in Ireland.

Did you visit northern Ireland on that 1956 trip?

Yes. After we had finished most of our work in the Republic of Ireland, Jane was anxious to visit northern Ireland, particularly the area where her grandmother and grandfather had lived before they immigrated to the United States in about 1870. She knew, at least, that her grandmother [Sarah Jane MacMechan] had lived in Castlewellan, County Down. This would be about thirty miles south of Belfast—right near the Irish Sea—and only about thirty or forty miles north of the border between the Republic of Ireland and northern Ireland. Castlewellan is near a much larger seacoast town, Newcastle.

Did you locate some long lost relatives?

On the first trip we didn't locate any relatives, but we did locate people who were familiar with the MacMechans. We visited the post office where Jane's grandmother had worked as a young girl and we visited Castlewellan, the huge estate two or three miles out of the village of Castlewellan. During the construction of the castle, Lord Ainsley had arranged for Jane's great grandfather [David MacMechan] to stay in the castle as a watchman. Apparently he served as the head gardner for the estate and planned and planted many of the garden areas on the castle grounds.

When we went back on a later trip [in 1970], Jane was able to locate some distant relatives and to find people who knew where her grandmother had lived and had word of an aunt who had lived there until her very recent death. We also visited the cemeteries where these relatives were buried.

Were there other trips or interesting things about your relationship with the Irish sugar industry?

Yes. We visited Ireland again in 1968, the year of my retirement, and spent a short time visiting with representatives of the Irish Sugar Company. They asked if I could come back to Ireland in the spring of 1970 and spend a few months working with them on their whole procedure of seed processing and seed treatment. That happened to be the year in which you were spending a few months at Broom's Barn in England and you and Skip [Mrs. Hills] found time to visit us in Ireland.

Right. I can certainly remember taking a trip with you and members of the Irish Sugar Company through southern Ireland and some of our experiences with the Irish people. It just used to amaze us to sit in one of their homes in the evening and hear the stories they would tell, using the brogue in which they would speak. We would think we were watching some sort of an American movie of the Irish people and it was very heartwarming and very interesting.

I might add that in 1976, the Irish Sugar Company published a book on the history of the sugar beet industry in Ireland and on page 62, they summarized the work on Phoma as follows:

> "Since the industry was started in Ireland [in 1926], the occurrence of black leg in beet seedlings due to a seed-borne organism was one of the main causes of low yields. Considerable experimental work was carried out to find a remedy. But no appreciable success was achieved until in 1951, the sugar company brought in Dr. Leach, a California expert, who worked out a dip treatment which was applied to all seed. It would probably be no exaggeration to say that the conquest of this disease was responsible for increasing the average beet yield by one ton per acre."

That's quite an increase over the number of years it has been since then.

And when you consider that their yields prior to this time were of the order of eight to ten tons per acre, this represented quite a percentage increase.

SOUTH AMERICA IN 1954: URUGUAY

Lysle, we've been talking about some of your foreign assignments, particularly with regard to Ireland and Denmark, but I'm quite sure that you had other foreign assignments about this time. Can you tell us about some of these?

After those first ECA assignments in Denmark and Ireland, the next contact that I had from a foreign source was in December 1953, when I received an airmail letter from Uruguay asking if I would come to Uruguay for about a month to serve as a consultant for the RAUSA [Remalacha Azucararus del Uruguay], a sugar beet company located in Montevideo.

Their agricultural superintendent, Mr. Jorge Spengenberg, had visited Davis previously and he was quite familiar with my work with Sclerotium root rot. It appears that this was one of the most important disease problems that they were experiencing in Uruguay and they wanted me to come down during their summer season, which would be December–January, to observe the disease in a number of fields and to advise them concerning a research program that would help to reduce the severity of this disease under their conditions. It happened that just at this time Jane had already departed for Montreal [Canada] where our daughter Joann and her husband, Dick Larkey, were living while Dick attended the medical school of McGill University. Joann was expecting her first child late in December, so Jane had gone ahead to be with them in Montreal during this period.

I found that at very little extra cost, I could fly to Montreal and down the East Coast of the United States to Montevideo, then return by the West Coast of South America. So that was the plan that I finally selected. Because it would include the Christmas vacation and I would only be away from work during the month of January, I had enough accumulated vacation time to cover this period. With the permission of the administration, I left Davis just before Christmas of December 1953 and flew direct to Montreal where I spent a few days visiting Jane, Joann, Dick and getting acquainted with our first grandchild, Diane, who was born on December twentieth. I then left Montreal one morning when it was about seven degrees Fahrenheit, flew direct to New York and changed planes for Caracas, Venezuela. When I arrived at Caracas at about six o'clock that evening the temperature was eighty degrees, so I found that I no longer needed my overcoat and other heavy clothing. We left Caracas about eight o'clock in the evening and flew all night over the Amazon jungle, arriving at Rio de Janeiro the following morning. We were on a DC-6B and this was quite early in the period of long-distance flights, so I was somewhat concerned about this long nonstop flight over the jungle with no possibility of intermediate landings.

From Rio, I proceeded to Montevideo and arrived there just before New Year's. I met Mr. Spengenberg and he laid out the plan for our work during the month of January [1954]. Because of the holidays, nothing would be done until after New Year's Day, so we went to the race track and enjoyed an afternoon of horse racing. That evening we returned to the hotel, the Victoria Plaza, which was the finest hotel in Montevideo. I had a very nice room on the twenty-first floor, overlooking the city of Montevideo and the harbor beyond.

Following the races I went to my room to change for dinner. A rather sudden electrical storm developed and all of the lights in that part of the city went out. When I was ready to take the elevator back to the lobby where I was to meet my friends, I found that the elevators were not in operation. So I had the pleasure of walking down twenty-one flights of stairs to the main floor, the only lighting being from their emergency generators. This was my first day of introduction to Montevideo.

After the New Year's, we moved from the city of Montevideo to a beach resort about sixty miles east where a very nice resort called Bella Vista was located, only a hundred yards from the South Atlantic.

Well, you've arrived at the seaside resort of Bella Vista. What happened then?

During the next several weeks, each weekday, Mr. Spengenberg would pick me up at about 8:30 in the morning and we would visit fields in different areas where sugar beets were grown. The company, RAUSA, has two sugar factories, both rather small in size, and they grow sugar beets primarily east and north of Montevideo over a rather wide area. During the day we would visit growers' fields where this disease, the Sclerotium root rot occurred, and would make observations on the conditions that led to the infestation. We also made an attempt to collect soil samples that could be washed through a series of screens to measure the population of sclerotia present in the field. But in this instance, I ran into real difficulty because the soil was almost a clay adobe. It was very difficult to get the soil tube into the soil sufficiently deep to collect a reasonable sample. After I collected the samples, I found it was very difficult to wash them through the screens because the soil would coat the screens in such a way that it was almost impossible to wash away the fine soil particles. I had to conclude that even though soil sampling was quite a successful method for measuring the population of the fungus under California conditions, it was going to be a very difficult problem to do the same thing in that part of Uruguay, where the soil was of this heavy clay type.

Well, what about the climate of Uruguay—is it very favorable to the development of Sclerotium root rot?

Uruguay is along the south Atlantic and has a very mild climate. Their winter temperatures are seldom below freezing and rain occurs almost every month of the year, although for certain periods they may be without rain for as long as three weeks. The temperatures during the growing season are very favorable for Sclerotium root rot and one of the main problems was that most of the growers had only very small farms and it was very difficult to rotate with other crops in a satisfactory manner. So the tendency was for the growers to grow beets one year after another, until the population of the sclerotia became so high that it was no longer profitable to continue with sugar beets.

Another problem that became fairly clear was that their beets were deficient in nitrogen. By the general top appearance it was quite clear that they were not applying enough nitrogen fertilizer to secure optimum yields or to minimize the severity of Sclerotium root rot. In general, conditions were very favorable for severe infestation.

It certainly seems obvious that sclerotium was a big problem in Uruguay. What about other root rots, did they have other kinds of things that we see here in addition to sclerotium?

Yes, there were additional problems and they were often confused with the Sclerotium root rot problem. For example, in a few fields we found violet root rot, a disease that I have never seen in California, but one that does occur in some parts of central United States and parts of Europe. Rhizoctonia root rot also occurred to a limited extent, but probably the most common disease problem affecting roots other than Sclerotium root rot was the wet root rot condition, usually induced by very wet soil conditions and caused primarily by water mold fungi.

After we had been there for about two weeks, Mr. Spengenberg decided to hold a clinic where growers could be instructed concerning the different kinds of root rot diseases that would affect sugar beets and how they could be recognized. So he invited the growers from all over southeastern Uruguay to collect samples of any rotted roots that they could find in their fields and to bring them to a small theater that was located near one of the sugar factories. Mr. Spengenberg asked me to give oral descriptions of the most common diseases affecting sugar beets and to show slides illustrating their characteristics.

How was your Spanish at this time?

Very limited. A translator was required. After my presentation, he invited the growers to come forward with samples of rotted beets which were placed on a table and it was my job to examine them, to cut the roots with a knife, and to explain what diseases were involved and how they could be recognized in contrast to other root rotting diseases.

We were somewhat overpowered by the number and diversity of samples that arrived and on my part it was a real workout to examine all of the samples and to give at least a partial explanation of what was happening to the beets.

Well, you certainly had many root rot problems, how about other sugar beet diseases?

During one of the visits to a field east of Montevideo, I was very surprised to find sugar beets infected by curly top, a virus disease that was very common in the western United States. This virus disease had been recognized in Argentina and had been studied by Dr. C. W. Bennett, when he was there a few years earlier. He had also found the same, or a very closely related form of curly top in Brazil but had not observed it in Uruguay.

This form of curly top, according to Bennett's research, was very similar to curly top as we see it in the western United States, but was carried by a different vector from the leafhoppers that carry it in North America. When he tested varieties of sugar beets that were resistant to curly top in North America, he found they were also resistant to the Argentine form of curly top and this offered a possible solution in Uruguay, which is the only South American country east of the Andes that is currently growing sugar beets.

In one field we visited, the grower asked us to walk with him out to the center of a large field. There he showed us a small plot, four rows wide and about a hundred meters long where the growth of the beets was very much smaller than all of the surrounding beets. He explained that the sugar company required him to buy phosphate fertilizer and to apply it to his crop but he had some question whether he was really getting his money's worth. So he conducted his own little experiment by turning off the fertilizer applicator on four rows of beets a hundred meters long in the middle of the field where they could not be seen by any visitor. After seeing the difference in the growth, he was convinced that the sugar company was not taking advantage of him by requiring him to pay for this fertilizer.

In another field, a leaf-eating insect had removed all of the leaves except the midribs and a few of the larger veins. The field no longer looked like sugar beets but more like pineapples. The grower explained that he was not really concerned that the insects were eating all of the leaves off of his beets because he was only growing them for the roots and as long as they didn't eat the roots, then it was of no concern to him.

Now, as a result of this fairly short visit to Uruguay, what were your final conclusions?

Just before I left Uruguay they asked me to come to the headquarters of RAUSA in Montevideo and report my results to the top officials of the company. There I explained what we had found in our field observations and recommended very strongly that an attempt be made to avoid repeated plantings of sugar beets in the same field, but rather to rotate with nonsusceptible crops for periods of three or four years wherever possible. I also recommended that an attempt be made to apply nitrogen fertilizer to the crop because it was obvious that most of the fields would be more productive if they received adequate nitrogen supply and that this would also result in a reduced severity of the Sclerotium root rot.

After the meeting in Montevideo, I learned that there was one additional bit of information that they wanted to get from my visit. They had decided that they were very much interested in building an additional sugar factory in a new area. And one of the things that they wanted me to do was to visit that area and determine whether Sclerotium root rot was severe enough on the sugar beets that were being grown there that it would be a serious handicap to the successful development of a sugar factory. If I had realized that an investment of six million dollars hinged in part on my observations, I think I would have been a little more cautious about my observations and conclusions. In any case, the factory was never built.

Well, after this visit and returning to the United States, what about your experiences in Uruguay since then. Can you give us some idea how they managed this problem up to the present time and how things are going there?

Since that time, they have continued to grow sugar beets and, in fact, there are two other companies producing sugar beets in Uruguay, but they are primarily in the west part of Uruguay near the Uruguay River. Those areas also have severe Sclerotium root rot problems, but the soils are much lighter and somewhat easier to manage. And also, there's a greater abundance of suitable land and it is much easier to rotate crops in that area.

In the territory of RAUSA, they still have a considerable problem with Sclerotium root rot because of the difficulties of maintaining suitable rotations. But they are using a great deal more nitrogen now, are getting somewhat better yields, and in recent years they have undertaken quite extensive research studies on methods of control that are most suitable for Uruguay.

For example, a few years ago they applied to the Food and Agricultural Organization, a branch of the United Nations with headquarters in Rome, for an investigator to be sent to Uruguay to again look into this problem and to plan types of programs that their own research personnel could continue. This program is now underway and hopefully they will be able to develop a more satisfactory program for minimizing the losses from this rather serious disease.

After your work in Uruguay was finished in early 1954, did you return directly to California or did you visit some other places?

BRIEF VISITS TO ARGENTINA, CHILE AND PERU

I had only a few days more that I could spend away from Davis. But I did take advantage of the opportunity to stop in Buenos Aires for two days and there I had a chance to get well acquainted with the city and met some very good friends. I was entertained extensively by Tom Williams, a very successful industrialist. He was a Kappa Sigma from the University of Arkansas and he arranged a luncheon at the Alviar Palace with several members of our fraternity, including one who attended UC Davis with the first group of degree students from Berkeley in 1909.

When I made my first trip to South America, I was also dean of students at Davis. A foreign student by the name of Pablo Fuentes had come to my office in about October of 1953 and told me that he had to withdraw from school and return to Chile because his uncle had died and it was necessary for him to take over his uncle's business. He said if I ever came to Chile to be sure and look him up. At that time, I had no thought of ever making a trip to Chile, but only about six weeks later I was contacted by the sugar company in Uruguay. As I've said, I was able to arrange my travel plans so that after leaving Uruguay I went to Argentina and to Chile and then returned by way of Peru. While I was in Santiago, I called up Pablo Fuentes, who was living in Vina del Mar. He very kindly came to Santiago and showed me all the sights of the city and we made a short visit to Vina del Mar before I left on the return trip home.

Didn't you visit a student's family in Peru also?

Yes. A student from Lima, Peru, by the name of Freddie [Fredrico] Uranga said that if I came through Peru to be sure and visit his folks. He wrote them to expect a visit during my return trip. I notified them when I would be in Lima and called them as soon as I reached the Hotel Bolivar. For two days they entertained me royally. They drove me on a trip high into the Andes. In fact, we intended to go over the Andes into some valleys beyond that which were occupied by Peruvian Indians. Senor Uranga thought this would be very interesting to me. Unfortunately, they were reconstructing the highway and we were not able to cross over the Andes, but did reach very high altitude before we returned to Lima. I had a very good introduction to Lima in the two days that I stopped over. Then the Urangas took me to the plane and I continued on my trip home.

After that trip, I returned directly from Lima to Davis and resumed my activities in the dean of students office and in the Department of Plant Pathology.

· XVII ·

FIRST, ONLY SABBATICAL LEAVE, 1956

Well, what came next with regard to foreign assignments? It seems to me you didn't take many sabbaticals but you took one pretty soon, didn't you?

Yes, in 1956, I took the only sabbatical that I have taken during the forty years that I was active in the Department of Plant Pathology. Other trips that we took during that time were shorter and they were either on reassignment, as in Chile, or they were taken on annual leave.

It was fairly uncommon for faculty members to take sabbatical leaves prior to World War II, wasn't it?

Yes. I can remember a number of people who did take sabbaticals, but I would say they were rather uncommon.

Mainly from a financial standpoint?

Probably, and it was difficult to get away because there were very few people that were available to continue the research and teaching while they were gone for a period of six months to a year.

Where did your sabbatical take you?

To Europe on an extended tour of experiment stations. Our eleven year old daughter Janet went with us this time. Jane and Janet went ahead by train and visited Joann and Dick in Montreal. I arranged to meet them in New York and we sailed together on the Maasdam, a Holland-American liner, direct to Cork in Ireland. We spent a few weeks visiting with the people we had been associated with on our previous stay in Ireland. We then purchased a car, an English Hillman which was very much like a small station wagon, what the English called a "shooting break," and took quite an extensive tour.

First, we crossed the Irish Sea by ferry boat to England and then by ferry boat from Dover to France. We then drove to Spain and spent about four weeks there, including a visit to the National Research Institute in a suburb of Madrid.

When you take one of these sabbaticals you usually have a definite purpose in mind. What was the general outline of your objectives of this particular sabbatical?

SURVEY OF EXPERIMENT STATIONS IN SPAIN, ITALY, SWITZERLAND, GERMANY

My primary purpose was to visit a number of experiment stations to discuss with their research workers problems of seedling diseases and root rots of field crops. In this way I was able to pick up a good deal of information that could be applied to the problems that we were trying to solve in California. I visited research stations in Spain, Italy, Switzerland and particularly in Germany. One of the most profitable visits was when I visited the biological research station of the Bayer Chemical Company at Leverkusen, just north of Cologne.

They were doing a great deal of work with new fungicides, testing them not only as foliar pesticides but also as seed treatments and for soil application. One of the new materials that they had developed was the fungicide that later became known as Dexon and one that we found to be very effective in controlling Pythium as a seedling pathogen of sugar beets. This compound was used for seed treatment with both sugar beets and with cotton.

I guess you could say that I owe my Ph.D. to Dexon because, as we already discussed, we did a lot of work with that.

Yes, it turned out to be a very interesting fungicide. And I certainly have enjoyed the time I've spent with you, Jack, in our investigations of some of these problems.

Okay, so now you're in Germany and you've found out about Dexon, what other things did you do on this particular sabbatical?

We took advantage of our time over there to travel to Denmark, where we renewed our acquaintance with many of the people that we had contacted in 1951. And we also travelled into Sweden, where we visited the research station of the Swedish Sugar Company at Landscrona, directly across the sound from Copenhagen. We then travelled north through the lake district of Sweden and then turned west to Oslo, Norway, and from there back to Copenhagen.

Eventually we visited England again and then sailed on the Maasdam back to New York.

Did all this ocean travel result from the fact that Jane isn't a very good flyer?

Well, that was part of it, but during most of this trip we had a car with us and naturally it was much easier to travel by ferry or steamer and have the car ready for us on arrival than it would have been to fly and then wait for it to arrive by freighter.

By this time Jane had made a few flights by air and had reached the decision that if she was going to make some of the trips that were planned, the only way would be by air.

What about the Hillman, did you bring that back to the States with you?

We brought it back to the States and drove it all the way across the country. Then the following day after our arrival in Davis, we turned it over to Joann and Dick, who drove it back to Montreal with their two children and used it there for a year. It was finally returned to California and then we turned it in on another car.

On your return from this sabbatical in 1956, I suppose you came back to the dean of students office.

Yes, I did and I continued my activities there until 1958. By that time it was clear that Chancellor Freeborn would be retiring soon and I decided that it would be an opportune time for me to ask to be relieved of my duties in the dean of students office so that I could return full-time to plant pathology. This was formalized at the end of June 1958.

During the next two years I continued working primarily with seedling diseases of sugar beets, beans, cotton and other field crops. In 1960, Dr. Kendrick retired as chairman of the department and I was asked to assume that position.

Shortly after that we had a visitor from Chile who wanted to discuss a serious seedling disease affecting sugar beets in Chile. The visitor was Mario Vallejo, who had formerly worked for the Rockefeller Foundation and had completed his graduate studies for a master's degree in nematology at the University of Wisconsin. He was then employed by the IANSA, the National Sugar Company of Chile, and was concerned with the technical aspects of sugar beet research. He described the seedling disease and indicated that they had not been able to determine the causal agent and that it did not respond to ordinary seed treatments. He said it was very important that they determine the cause and a suitable control. On the basis of their description, I suggested that it probably was not caused by Phythium or Rhizoctonia, and it probably was not the seedborne Phoma. The only disease of seedlings that we have in California that seemed to fit their description was the water mold seedling disease caused by a fungus called Aphanomyces.

He took notes on this conversation, returned to Chile, and made arrangements for a student in the University of Chile to undertake an investigation of this disease. But the student was unable to isolate Aphanomyces and therefore concluded that it probably was not involved in their problem.

· XVIII ·

ASSIGNMENTS IN SOUTH AMERICA, 1962–1976

In the late summer of 1962, I received a communication from the dean of agriculture in the University of Concepcion in Chile. This was Dean Luis del Villar. He had requested funding from the Rockefeller Foundation to make it possible for me to spend about three months in Chile advising with him on the academic programs that they were conducting in the University of Concepcion.

The faculty of agronomy of the University of Concepcion is located at Chillan?

Yes, the main university is at Concepcion near the coast, about 250 miles south of Santiago, but the College of Agriculture is at a town called Chillan, located about sixty miles inland and directly on the Pan-American Highway, south of Santiago. Dean del Villar also indicated that he hoped that I would find time to look into the seedling disease of sugar beets that they called *caida.*

Caida *in Spanish meaning "falling over."*

Yes, that was the typical description given by Mario Vallejo and by others: the diseased seedlings would fall over and would perhaps survive in a very weakened condition. So that the yields from fields that were seriously affected were very much reduced.

ROCKEFELLER FOUNDATION GRANT:
ADVISOR TO UNIVERSITY OF CONCEPCION, 1962

This application was approved by the Rockefeller Foundation. Jane and I tried to learn as much Spanish as we could on short notice and we flew to Santiago in September 1962.

Janet, then a senior in high school, stayed in Davis with Joann and her family who had settled there when Dick began the practice of medicine the previous July. Once we got to Chile, we proceeded to Chillan by train.

You arrived during their springtime.

Yes, September in Chile is early in the spring. We were met by Dean del Villar at the train and he took us to lunch at the hotel where we were to stay during our visit in Chillan. Following lunch, he took me to the university and almost directly to the greenhouses where two students were studying the *caida* disease as their thesis problem for the degree Ingineer Agronomo. From the characteristics of the infected seedlings, I immediately indicated that I was certain that this disease was the same as the one that we called the water mold seedling disease caused by Aphanomyces. I said that if they would permit me to collect a few typical seedlings, I could place them in water culture overnight and we would certainly have a definite answer. This was arranged and I selected about twelve seedlings which seemed very typical to me. We took these to the laboratory to incubate them for identification of the pathogen. Dean del Villar apologized for the fact that their sterilizer was broken and therefore they had no sterile water, nor did they have any sterilized petri dishes. I assured him that this was no problem because I could readily wash a few petri dishes and we could use the ordinary tap water for the incubation of the seedlings.

The following day I examined the seedlings in the laboratory under the low power of a compound microscope and found that all of the seedlings were showing sporulation typical for the fungus Aphanomyces.

This must have been a shock after all the time they'd spent trying to find the answer for some guy to come down and just do it overnight.

Yes, this was a little surprise to them as well as to me that it proved to be so simple. Previously, in 1960, Dr. Winner had been sent by the government of Germany to study this disease and he had also arranged for a hundred kilograms of soil to be shipped to Germany where he had worked with these soils for two years. Dr. Winner returned to Chile in 1962 and was there on his second trip during the time that we were at Chillan. He had recovered a species of Pythium that was pathogenic to seedlings, but he assured me that he had never found Aphanomyces in any of his isolations. After I showed him the sporulation of this fungus from the seedlings, he agreed that Aphanomyces appeared to be the dominant pathogen.

Why do you suppose it was that he hadn't found Aphanomyces?

I think the problem was that it was common in those days to sterilize the plant tissue with mercuric chloride to eliminate secondary or extraneous organisms. I had repeatedly found that if you used mercuric chloride to sterilize the tissue, you very seldom would ever recover Aphanomyces, although you might recover other pathogens such as Pythium or Rhizoctonia. I believe that this is the reason that he had not found this particular organism. Also, this organism does not sporulate directly on agar, although it grows in abundance as vegetative mycelium. It would be impossible to identify the organism directly on potato dextrose agar, whereas placed in a thin layer of water, the fungus sporulates readily and it is so characteristic that it is quite easy to recognize in contrast with other pathogens.

Following this diagnosis, we collected soil from many fields in Chile where the *caida* disease was known to occur,and in every case, we found an abundance of Aphanomyces. In some fields, however, other pathogens such as Pythium and Rhizoctonia were also abundant and I think this accounted for the difficulty in identifying the organism and in understanding why these specific symptoms occurred.

In 1960 Dr. Winner had brought samples of a number of fungicides to Chile which he tested by mixing them with the soil in a field where *caida* was very severe. Among six or eight fungicides, the only one that proved effective as a soil fungicide was Dexon and arrangements were being made to introduce Dexon into the seed furrow along with a phosphate fertilizer. In preliminary trials, this showed very good control of the *caida* disease, even though at that time they did not know the primary pathogen.

In discussion with him, I asked how he had arrived at the dosage which was so effective because in his trials he used only a single dosage. He laughed and said, "Perhaps you remember when I was visiting you in Davis in 1958, you took me to your greenhouse and showed me your trial of soil treatments with Dexon, and I put the dosage in my notes. So when this problem came up, I used the same dosage here that you had shown me in Davis."

It shows that world travel sometimes does some good.

It was a real coincidence that the work that we had done in the greenhouse here at Davis was eventually translated into identification of the true pathogen in the case of the *caida* disease and to development of control measures.

Another point related to our research at Davis was that in the research reports from IANSA, I noticed they found that while Dexon as a dust introduced into the seed furrow effectively controlled the *caida* seedling disease, the same fungicide in solution or suspension was ineffective. Since this result was not in conformity with our experience, I asked Gaston Gonzalez who had conducted the trials, "What was the color of the solution of Dexon when you applied it in the seed furrow?" His reply was, "It was the color of red wine." From this I knew that the fungicide had decomposed after exposure to sunlight and thus was no longer effective as a fungicide, as you, Jack, had previously shown in your thesis research.

Very interesting. (Laughter)

Well, while you were in Chillan, Lysle, did you do some more greenhouse tests on the effects of Dexon on Aphanomyces and then anything in the way of field trials?

I did not have time for field trials except for the collection of soils and identification of the pathogens that were present. But I did carry out a number of trials in the greenhouse. I was struck first by the fact they had decided that to control *caida* it was necessary to use from one and a half to two kilograms of Dexon per hectare (2.47 acres) when applied with the phosphate fertilizer in the seed furrow. According to my experience, this was far more than was necessary because satisfactory control can be secured with a concentration of about twenty-five parts per million in the soil and this would require only about a half a kilogram per hectare.

One half pound per acre or something like that.

Yes, about half a pound per acre. It was explained to me that they did not feel they could reduce the dosage because in many of their fields they still had a certain amount of seedling disease. But I was able to demonstrate that the disease that occurred following the Dexon soil treatment was primarily caused by Rhizoctonia and that this fungus could not be controlled by any dosage of Dexon. In the fields I observed there were several cases of severe phototoxicity in which the seedlings were slow to emerge, were dwarfed and did not grow vigorously. This was typical of what we had observed from overdosage in the use of Dexon. So I was finally able to convince them that for the control of *caida,* it was not necessary to use such a high dosage of Dexon because a dosage of one-half or less would be all that was required. To secure satisfactory control of other organisms such as Rhizoctonia, it would be necessary either to use a seed treatment with a material such as PCNB that would control this fungus, or to use a soil fungicide that was effective against Rhizoctonia.

How did this work that you and Dr. Winner had done in Chile tie into the actual application of these findings to commercial agriculture?

Mr. Segrid Arenson was in charge of the research on seedling diseases for ANSA, the national sugar company, and his assistant was Gaston Gonzalez. They were the ones who conducted the field experiments leading to the use of Dexon in the seed furrow. During the time I was in Chillan, they made arrangements for Gonzalez to come to Chillan and to work with me in order to gain experience on the identification of the organisms and on the conduct of greenhouse experiments.

Later, he collected soil samples from various areas of Chile all the way from Puerto Montt on the south to Santiago on the north—a distance of 700 miles—and grew seedlings in these soil samples in a greenhouse at Los Angeles, Chile. From these trials he learned that Aphanomyces occurred in almost all areas and that the fungus was often abundant in soils that had never previously grown sugar beets.

A few years later, Gaston Gonzalez came to Davis and carried out studies in plant pathology leading to the master's degree, and completed many additional courses before his return to Chile. He then conducted a large number of additional trials in the greenhouse and in the field and finally published an extensive bulletin compiling all of the research that had been done on the *caida* disease over approximately a ten year period.

What happened to you next in Chile after doing all this greenhouse work? How long were you there actually and what finally happened?

Well, I was in Chillan for nearly three months in 1962 and during this period I spent a good deal of time with Dean del Villar discussing their academic program and attending a number of faculty meetings where questions involving grading of courses, reporting of academic records for students who were applying for graduate studies elsewhere. Such matters as faculty organizations, promotion and retirement systems were discussed. I was involved in a general survey of their academic program and I think they were quite interested in learning how these activities are carried out in the University of California.

HONORS IN CHILE: MIEMBRO ACADEMICO

Near the end of my stay in Chilan, Dean del Villar told me that upon his recommendation I had been elected "miembro academico" in the Faculdad Agronomia of the University of Concepcion. This is an honor that is usually reserved for senior or retired members of the faculty and had not previously been granted to anyone in the College of Agriculture.

That was quite an achievement.

Well, I considered it a great honor, as a foreigner to be honored in this way. The vice-rector of the University of Concepcion came to Chillan to present the award and this resulted in a meeting attended by a large number of the faculty and other interested people. I was notified that I would be expected to give a response which could include a discussion of my work here at Davis or at Chillan. I made a very serious attempt to make this presentation in Spanish, but after studying with a translator for a number of days, it became very clear to me that it was not going to be a very satisfactory presentation. I ultimately settled for my introductory paragraph in Spanish, followed by the lecture in English with simultaneous translation. The meeting was followed by a very elaborate dinner at the hotel and for Jane and for me it was quite an interesting occasion.

Also during the time that we were at Chillan, Jane and I were very interested in the stories of very large Chilean rainbow trout, and we inquired how a person could make arrangements to try fishing in the lakes of Chile. Eventually, we met a man by the name of Gonzalo Leiva who seemed to be the best authority on fishing. He invited us to go on fishing trips at various Chilean lakes and rivers on weekends several times during our stay at Chillan and we not only were very successful in catching a number of Chilean trout, but these trips proved to be outstanding social affairs as well because they always included typical Chilean barbecues and very interesting experiences.

What was the largest trout you caught?

Left: Lysle Leach and F.J. Hills participating in the first meeting of the Latin American Sugar Beet Technologists, Bella Vista, Uruguay, December 1976. *Right:* Greenhouse research, University of Concepcion, Chillan, Chile, October 1962.

I think the largest one we caught was about two pounds, but I saw a ten pound rainbow trout caught in very swift water in the Laja River. It was caught on very heavy fishing equipment and the fish took the fisherman downstream for at least half a mile before he was able to subdue the fish and bring it ashore.

I will always remember the couple of times I went fishing in Chile. The tremendous streams they had to fish in and really the few number of fishermen. Gosh, if those kinds of streams were up here, why, we'd all be out there fishing. (Laughter) Well, Lysle, it seems to me that it wasn't very long after this 1962 trip you took to Chile, that there was some sort of a Chile–California program, first with the state of California, later with the universities–cooperative programs between the two governments and then the two universities. Were you involved in any of these activities?

FORD FOUNDATION: UC–UC EXCHANGE PROGRAM, 1965

I was not involved in the state of California–Chile program, but I was quite involved in the program that was supported by the Ford Foundation for an agreement between the University of California and the University of Chile for exchange of personnel and for graduate training of selected students. I was asked to serve on the organizing committee as a representative for plant pathology statewide. Our first trip on this project was in October 1965, when we went to Chile to meet with the dean of agriculture and other faculty members at the University of Chile and to plan the program as it was to develop.

I remember that time well because I had just gone to Chile to start a sabbatical leave with the idea of studying the yellow wilt disease of sugar beets as a sabbatical activity. You came down shortly after that, and so we were together there for some time.

Yes, I visualized that the work you were planning to do in Chile would fit very well with this Chile–California program. But it developed that the University was not involved in sugar beet research as most of that was being done at the National Research Station located at La Platina.

But I'm sure that during your stay there you met many of the faculty of the University of Chile and were involved in a number of programs that these people were working with.

Yes, even though the university wasn't directly charged with working on this problem, I was able to work with several of the faculty members and we had a pretty good relationship. But tell us, Lysle, about some of the other developments in this trip to Chile and your role as the plant pathology leader.

The purpose of the trip was to reach an agreement with the committee members employed by the University of Chile concerning the people who would be involved in exchanges and how we would proceed to provide assistance for the research workers located in Chile. We found rather quickly that the administrators in the University of Chile did not want to depend upon us to select the people that we thought would be most useful to them. They felt that they were quite familiar with the research being conducted in California and it was their wish to select people that they felt could be most useful to them.

During the next few years we were able to provide a considerable amount of assistance to the University of Chile. First, we assisted in building up their library; second, by providing laboratory equipment, even to the extent of a greenhouse that was shipped and assembled at their experiment station. We also sent a number of staff members, primarily from Davis and

Riverside, who were working on projects that were of interest to faculty members in the University of Chile. Most of their interest seemed to be in the area of fruit diseases, both deciduous fruits and citrus. They were not as anxious to bring people down to work in the area of field crops or vegetable crops.

How long did you stay in Chile during this 1965 trip, and what were some of the subsequent developments?

I spent about four weeks in Chile at that time. I then returned to Davis and over the next two or three years, several graduates of the University of Chile were selected by their faculty to come to Davis and to Riverside for graduate studies. These students were, in general, well prepared for graduate studies and completed their work here in a very satisfactory manner. Upon their return to Chile, most of them were employed by the University of Chile. However, there were political problems in Chile shortly after that and our disappointment has been that very few of them still remain as teachers in the University of Chile, many of them having accepted other positions outside the University or outside of Chile.

Have you been back to Chile since your 1965 trip?

OAS MASTER'S PROGRAM IN CHILE, 1967

Yes, I've been there on two occasions. One was in 1967 when I was invited to teach a course in plant pathology. A number of students from Chile, Argentina and Uruguay had been selected by the Organization of American States to attend a master's degree program in genetics and plant improvement. As a part of this course it was decided that they should have three courses in plant pathology.

One would be an introductory course as a review, the second one would be a course on diseases of special crops and the third on methods in plant pathology. I was invited to teach the introductory course. When I arrived in Chile, I met with the organizing committee and asked them who they had selected to teach the other two plant pathology courses so we could coordinate our materials. The chairman indicated that that was a problem to them because they had no one else and they were wondering since I was there whether I wouldn't arrange to teach all three courses concurrently.

That's asking quite a lot—to teach three courses like that.

Well, this meant that I was a very busy person for a period of about ten weeks because I had to drive about twenty miles from our apartment to the research station where the course was being given. I tried to go early to avoid the very heavy car and truck traffic. Then I was busy from morning until late in the afternoon with the laboratories and in the preparation of materials for the following day. This was probably the most concentrated teaching effort that I have ever undertaken and I was quite relieved when we reached the completion of the courses.

I'll bet.

But there were some rewards because the Organization of American States, during the middle of the course, sent us by car and bus on a trip to southern Chile. During this trip we visited a number of experiment stations and areas of production of different crops. Then at the termination of the course, they arranged for the entire class and instructors to fly over the Andes to Mendoza in Argentina where we spent five days visiting a number of experiment stations and the University in Mendoza. This proved to be very interesting, both for the students and for me, so looking back on it, it was a very worthwhile experience, considering all aspects.

The only other time that I've been to Chile was on a personal trip. When returning from Uruguay in 1976, we flew to Buenos Aires and south to Bariloche in the Argentine lake district. We then crossed through the Andes by boat, across the lakes, and by bus over the mountain passes until we reached southern Chile. There I spent two days fishing with a friend in the Rio Bueno, a very beautiful stream south of Valdevia.

This was Gonzalo Leiva again?

Yes, this was with Gonzalo Leiva. He had been working in La Union a couple of years before and had relatives there. So he came down from Santiago by train with all the fishing equipment and spent the two days with me on this fishing trip. Following that we took the train from La Union into Santiago and spent a few days visiting friends from previous trips and then returned to Davis.

· XIX ·

ADMINISTRATION AND RESEARCH AT UC DAVIS

Lysle, as department chairman I suppose you were very busy with administrative activities and didn't have as much time for research, but can you tell us a little about what did happen as far as your research activities were concerned while you were department chairman.

Well, as you suggested, the time available for research while I was chairman of the department was somewhat limited because of the amount of time that was required for administrative meetings, for preparation of budgets, plus all the reports required to keep the department operations at an effective level.

However, I was able to continue some of the work that I had previously done involving seedling diseases of sugar beets and other field crops. I particularly studied the effect of climatic factors on the severity of infection of seeds of sugar beet by *Phoma beta,* the only important seed-borne pathogen of sugar beets.

STATEWIDE RESEARCH ON VERTICILLIUM WILT IN COTTON

In 1961 I was appointed chairman of a coordinating committee to supervise the statewide research on the very urgent problem of verticillium wilt on cotton. This involved laboratory and field studies of over twenty staff members from the Berkeley, Davis and Riverside campuses of the University and also the activities of USDA personnel located at Davis and Shafter and a number of people in the Cooperative Extension Service.

It must have been quite a group to handle.

It was. We found that the committee was not effective in directing the research since that was largely up to the individual who volunteered to participate. But it was our job to plan each year's activity, provide the field facilities that were required, and at the end of each season to collect reports from all of the people involved and to put out an annual report which was then provided to growers and other interested people, particularly those who were supporting the research by various staff members.

How many of these reports were issued by that coordinating committee?

I believe there were four.

What were they called?

They were called the Annual Report of the Cotton Research Committee. They reported what investigators from Berkeley, Davis and Riverside had been doing, and included reports of the USDA people who were involved and local reports by the farm advisors in the cotton-producing counties. They provided fairly complete reports on the work that was being done.

What came out of all this activity in cotton disease research?

I think the most effective area turned out to be the improvements in breeding for resistance. Dr. Fred [N.] Briggs, who had recently retired as dean of the College of Agriculture at Davis, was appointed as the University's representative. A well recognized agronomist, he worked with plant breeders at Shafter [Dr. Cooper and John Turner] in the improvement of the resistance to verticillium wilt until his unfortunate death in a car accident in 1965. The breeding program has continued under a somewhat different organization and at the present time varieties are available that are considerably more resistant than the ones that were used in the 1960s. The question of how useful those reports were to the farmers is a little harder to evaluate because the primary control for the verticillium wilt problem has been the development of resistant varieties. Considerable progress has been made in this way, first by the USDA plant breeders and now by a private organization supported by the cotton seed producers and distributors.

What has been your involvement with cotton research projects in recent years?

Until my retirement in 1968, I cooperated actively with Dr. Richard Garber at Shafter in evaluation of fungicides for control of cotton seedling diseases. Since retirement I have not conducted any cotton disease research.

Who was involved in this cotton research at Davis?

Dr. William Schnathorst, a USDA plant pathologist, and Dr. Donald Mathre, a pathologist with the University, were both actively engaged in cotton disease research. After Dr. Mathre left the department his work was continued by William Reische, a laboratory technician, and by Mike Hoffman, also employed on this project. After 1966 when benomyl, a systemic fungicide, was introduced, we evaluated the possibility of using this material for the control of verticillium wilt. If we mixed 100 parts per million with soil and grew cotton in containers in this treated soil, we secured almost complete control of verticillium wilt for a six-month period. But when we tried similar methods in the field, we found the plants absorbed the benomyl, translocated it to the branches and leaves and protected against verticillium wilt for a period of only a few weeks. When the roots of the cotton plants extended beyond the zone of soil that could be treated with benomyl, then the amount of systemic fungicide in the above ground parts of the plant was gradually reduced and soon the plants were showing typical verticillium symptoms. So we concluded that while the fungicide was effective, it was not a practical way of controlling verticillium wilt because there was no possibility of incorporating the fungistatic concentrations in the total soil mass that would be occupied by the cotton roots.

It's still a problem.

Yes, the breeding of tolerant or resistant varieties is still the most promising procedure and considerable progress has been made.

In June 1968, when I retired, James DeVay took over the work on cotton diseases, both seedling diseases and verticillium wilt, and he has been continuing investigations along these lines since that time.

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POST RETIREMENT RESEARCH CONTINUES

Well, we know you retired in 1968, but having been a coworker with you it didn't seem to me that you retired. I know you've done a lot of research since then. How about some of these research activities since your retirement?

COMPILATION OF CLIMATIC STUDIES RELATING TO PHOMA

I've mentioned the work on relation of climatic factors to the severity of infection of sugar beet seeds by seed-borne fungus, Phoma. After retirement I found time to compile all of the information that we had collected and to continue until we had a fairly good picture showing that the severity of seed infection was closely associated with the amount of rainfall that fell during the maturity and harvesting of the seed crop. In years with abundant rainfall during this period, high percentages of seed infection occur and the fungus would be deeply imbedded in the seed units. In years with very little rainfall, before and during the harvesting period, the seed lots would usually show a lower percentage of infection and,most of the infection was superficial as mycelium or as contamination with spores.

The importance of this difference was that with superficial infection, the processing of seed as is normally done by removing the outer cork tissue also removed most of the infection and contamination, whereas a deep-seated infection could not be removed by this process. In the same way, ordinary seed treatments used as protectants against seedling diseases reduced the amount of infection when we were dealing with light infections but not with severe and deep-seated infections. We concluded that in most years the seed lots used in California were only lightly or moderately infected and, after processing and treating, could be planted under California conditions with the minimum possibility of serious seedling infection.

It seems to me that was a very worthwhile discovery because it takes a lot of the pressure off of the necessity for treating seed for Phoma *like they have to do in Europe.*

During this study of Phoma *infection of beet seed, you ran into some other problems in Oregon beet fields, didn't you? A disease they called stalk blight was giving them some trouble.*

FIELD STUDIES IN SOUTHERN OREGON LEAD TO DISEASE-RESISTANT BREEDING PROGRAM

Yes, in 1971 Sam [Samuel C.] Campbell, who was in charge of the production of sugar beet seed in Oregon, called my attention to the fact that certain fields, particularly those which were growing the new varieties, US H9 and US H10 for use in California, showed a considerable amount of the killing of stalks so that the plants would wilt before the seed was matured, and in that way the seed production was strikingly decreased.

Two visitors from other areas had identified this disease as having been caused by *Phoma,* but I soon found that there was very little correlation between the amount of stalk blight in the field and the amount of *Phoma* infection on seeds that were produced. I reached the conclusion that this must be caused by a different pathogen. So we collected material from Oregon and made isolations and consistently recovered a species of *Fusarium.* When these cultures were purified and used to inoculate seedlings in the greenhouse, we found them to be highly pathogenic and to produce on young plants very striking symptoms of vascular discoloration and systemic infection. The first isolations were made by a graduate student, Dennis Gross, but most of the research on this disease was with another graduate student, James McDonald, who carried out a great deal of the laboratory and greenhouse studies on this disease. In the field, we noticed that most of the infection occurred in fields planted to the varieties US H9 and US H10 which were produced primarily for use as seed in California, whereas most other varieties of sugar beets growing in the same general area had much less stalk blight. So we arranged with John McFarlane, who was in charge of the USDA program for production of new sugar beet varieties, to make a collection of different breeding lines and varieties, and these were all planted in a highly infested field near Salem, Oregon.

These trials showed that the male sterile lines that were used to produce the California hybrid varieties were very susceptible, whereas the pollinator used for these varieties was much less susceptible. We also found that most varieties which were not related to the male sterile strains that were used for these curly top resistant varieties, were relatively resistant. So it was concluded that during the breeding program of these male sterile lines, they had inadvertently selected breeding lines that were highly susceptible. To correct the problem, the USDA then modified its program and has since selected equivalent male sterile lines that show a fairly high degree of resistance. In the future, this disease will be avoided by the use of these new male sterile lines.

We also found that the pathogenic fungus was present in fields that had not previously been grown to sugar beets or garden beets. This suggested that there must be other hosts to maintain the pathogen previous to the planting of sugar beets, or between sugar beet seed plantings which are usually made at intervals of five or six years.

We found that common pigweed, *Amaranthus retroflexus,* was very susceptible when the plant reached the stage of seed production, but prior to this time little or no symptoms were evident. This was a weed host that could readily be the means of maintaining this pathogenic fungus in the absence of other susceptible crops.

We also tested spinach for seed production in this area and found that once the spinach reached the seed producing stage it was very severely affected by this organism. We have here a case where fusarium wilt was pathogenic on plants in several different genera of plants, whereas usually fusarium wilt pathogens are very specific to the species of a single genus of hosts.

By gosh, I think you fellows ought to name that fungus after me because I think I was the one who handed Jim MacDonald that pigweed plant and said, "What about this?"

I think that's right. I remember the incident in the field at Salem.

OUTBREAK OF POWDERY MILDEW DISEASE ON CALIFORNIA SUGAR BEETS, 1974

In 1974 we had an outbreak of a new disease in California, at least new from the standpoint of it causing important commercial problems. That was the disease that we call powdery mildew. I know that you have had some experience with this disease in some of your trips to Europe, and that since that time you've had a very important role in developing some possibilities for controlling these diseases in a far easier way than we can at the present time. So tell us something about this.

With the rather severe occurrence of powdery mildew in California and in other western states in 1974, Jack, I know that you were very active in testing for the use of sulphur compounds for the control of this disease and in determining the number and frequency of applications that would be necessary to control it.

I had talked with people in Germany in 1974 who told me of a new systemic fungicide that was very effective for the control of powdery mildew diseases, particularly on cereals. And I decided that it would be useful to evaluate the possibility of the use of systemic fungicides which could provide internal resistance to the plants, in contrast with the external control or protection that is provided by applications of sulphur dusts or sulphur sprays. In this work I had the assistance of a young lady, Carol Frate, who was a graduate student in plant pathology working for her master's degree. She took a very important part in conducting greenhouse and field trials over the next few years. At first we collected four fungicides that were reported to have systemic action and tested them in the greenhouse on sugar beet seedlings to determine which would be most effective in controlling this disease. It was quite clear that one of them, now called Bayleton, was more effective than the other three, so the following season we went to the field plots with trials using this material.

First, we were concerned about the method of application that would be most suitable for a systemic fungicide, so we tried seed treatment and incorporation of the fungicide in the soil below the seed and, additionally, side dressing with a granular formulation of fungicide in comparison with spraying of the fungicide on the leaves of the plant. During our first year in the field, we found that seed treatment was ineffective, while the soil applications controlled the powdery mildew but did not appear to have a high level of efficiency. So we considered other methods. The one we settled on was to introduce a granular formulation of the fungicide directly into the crown of the plant by scattering the granules directly down the plant row so they would strike the leaves of the sugar beet and be funnelled into the crown where they would be in close contact with new leaves as they developed. This proved to be a very effective way of applying the fungicide Bayleton to sugar beets. We found that a single application about the time that mildew first appeared would provide season-long control, using this granular application in the crown.

When we used the material as a spray on the foliage, it provided very good control for a period of about two or three weeks, but then it was necessary to repeat the application. We felt that a single application of the granular form might be a very suitable method of applying systemic fungicides. In later years, we found two other materials that were effective in controlling powdery mildew that were also systemic.

One of them proved not to be effective when applied as granules in the crown. But when the material was sprayed in a concentrated form into the crown, excellent control was obtained. So over the period of the last four or five years, we have demonstrated that systemic fungicides are effective in controlling powdery mildew on beets and that it makes a great deal of difference how these materials are applied on the plants. Jack, we have depended on your trials in the last few years to refine this approach and to learn which fungicide will be the most effective in securing season-long control by a single application of a systemic fungicide to the growing plants.

It seems to me that this work of yours has been very important in demonstrating that the systemic materials need to be applied in the crowns to be most effective. This is a result I don't think any of the chemical companies anticipated at all when they developed these materials, so these studies made a big contribution to the control of this potentially very serious disease.

We talked considerably about many foreign trips prior to retirement, but I know since retirement that you haven't been sitting at home all of the time. Tell us something about your trips abroad after retirement.

CONSULTATIONS IN EUROPE, 1968, 1970, 1974

In 1968, immediately after our retirement, we took a trip to Europe and attended the first International Congress of Plant Pathology at the University of London in England, where I presented a report on soil application of systemic fungicides. We also took this occasion to pick up a new Mercedes Benz car in Germany, which we are still driving. Jane and I then travelled through several of the European countries where I consulted with people who were involved in disease research, particularly as it related to sugar beets.

Then two years later, we were asked to return to Ireland for further evaluation of the seedling disease problem.

This would have been when?

In 1970. The Irish Sugar Company wanted an evaluation of whether it would be possible to discontinue the use of the ethyl mercury phosphate steep treatment, in view of the fact that the company was at that time growing most of its seed in drier climates around the Mediterranean, particularly in southern France or along the Adriatic in Italy. So Jane and I lived in an apartment in Dublin for a period of about four and a half months while I investigated this problem on their current seed that was produced in the Mediterranean area. We found, as we had in California, that with the lightly infected seed, it was very possible to secure adequate control of the seed-borne *Phoma* by the use of ordinary protective fungicides, following the processing of the seed to remove the external cork carrying with it much of the superficial infection.

Then in 1974, Jack, both you and I were invited to present papers at the annual meeting of the IIRB, the International Institute for Sugar Beet Research. This meeting was held in Brussels, Belgium. There I discussed primarily the climatic factors relating to the severity of infection by *Phoma* on sugar beet seed and the effects of seed processing and treating on the incidence of seedling infection. Seed-borne *Phoma* is considered to be the most important seedling disease in most of the northern European areas, so there was considerable interest in this subject.

I remember you also presented a paper perhaps you would want to mention.

Oh, I was asked to talk about sugar beet fertilization, particularly fertilization with nitrogen in California. I presented some of the more recent work we had done in this area and some of the ideas we were trying to develop as to how to tell farmers to fertilize more efficiently. I remember that meeting and all the simultaneous translation that went on—a much more elaborate meeting than what goes on when we have one in this country where everybody must talk in English. There you've got German, Spanish, French and English being spoken and you have to pick up your earphones to understand what's happening.

Yes, that was quite an interesting experience and I enjoyed it thoroughly.

Following those meetings you took a trip to see the people at the Bayer Laboratories and found out about this Bayleton material that we've subsequently used here in California. Isn't that true?

FAO–WORLD HEALTH ORGANIZATION MEETINGS: CONTROLS FOR USE OF TOXIC SUBSTANCES

Yes, that was involved in the same trip. About the same time we were invited to the meetings in Brussels, I had been contacted by the FAO in Rome to represent their organization in a joint session with representatives of the World Health Organization to be held in Geneva. This discussion was related to the use of mercury fungicides as seed treatments on cereal crops and the possible dangers from the poisoning of animals or humans from the consumption of this treated grain.

Of course, no one is supposed to eat that treated grain, but apparently they do by mistake occasionally.

Yes, when the seed is treated it is also colored with a dye that is included with the seed treatment so that anyone should know that the seed has been treated with a poisonous fungicide and therefore should not be fed to animals or consumed by humans. But over the years it has been found that whenever people are really suffering from hunger and lack of food, they are inclined to take chances, even though they are warned that the seed has been treated with a poisonous fungicide.

The most serious recent incident occurred in Iraq in 1971. Following two years of drought, the supply of wheat for planting was very limited. Also, the government officials in Iraq decided that they would like to introduce some of the new varieties of wheat that had been developed in Mexico so they could participate in what was called the "green revolution." As a consequence, the government of Iraq purchased about 75,000 tons of wheat from the Sonora district of Mexico and they requested that this wheat be treated with a fungicide so that it would be ready for planting the following spring in Iraq. This wheat was shipped to Iraq and was distributed throughout the country in the wheat growing areas and there was considerable publicity to the effect that this seed had been treated with a poisonous fungicide and should not be eaten or fed to animals.

However, the people in certain areas were very short of food and they were somewhat inclined to believe that the government had merely added a dye to make them believe that it was not suitable for consumption so that it would be saved for seed. Some of them even placed the wheat in tubs of water and observed that the dye disappeared from the seeds. In other words, it could be removed by washing. What they did not realize was that the dye was removed but that the mercury was still present on the seed. Some of them fed their grain to chickens and observed them for a period of time and saw no toxic symptoms and therefore concluded that it would be perfectly safe to eat it. So they began to use this treated wheat for the preparation of bread, and this appears to have been a large part of the food supply for the people in certain areas. No symptoms were observed for a matter of two or three weeks, but by that time some of the children began to show unusual symptoms such as lack of coordination and later loss of hearing or sight. Where the consumption of the wheat continued for an extended period of time, many of the people became very ill and were taken to hospitals and there were many deaths. The government figures show that during this entire incident, between 450 and 500 individuals died as a result of this mercury poisoning. These records were taken from people who were in hospitals and show that an additional 8,000 or 9,000 people were poisoned to different degrees resulting in something less than death.

This happened before this conference in Switzerland.

Yes, this had happened in 1971 and 1972. The conference was held in 1974, primarily for the purpose of discussing how such a severe incident of poisoning food could occur and how it could be prevented in the future. So as I mentioned, representatives of the FAO and the WHO met in Geneva for about four days and discussed all aspects of this incident and particularly attempted to arrive at recommendations that would prevent this type of poisoning from occurring in the future.

What was your role in these discussions, Lysle?

I was involved primarily from the standpoint of the need for seed treatment on wheat to prevent the introduction of seed-borne diseases that would reduce the crop, or the possible introduction of more severe races of some of these pathogens than the races which already occurred within a country.

It was very clear that something should be done to prevent introduction of such a large amount of treated wheat that might be diverted for consumption even though warnings were given. We concluded that the methyl and ethyl mercury compounds used as seed dressing were so toxic that it would be very desirable to limit the use of this treatment to seed stocks that were being increased or multiplied for seed purposes *only* and should not be used on the seed that was ultimately planted by the grower.

I argued very strongly that seeds that were being exported, particularly to developing countries, should not be treated in the country of origin but should be shipped as untreated seed and then arrangements for the treating of the seed, where required, should be as close to the area and the time of seeding as was possible under given conditions. This would require a great deal of supervision on the part of the importing country in order to supervise the local application of fungicides to large quantities of seed. But I felt it was a much safer procedure than the treating of seed at the point of origin, particularly with the toxic types of seed fungicides. Not all of the other representatives agreed with this point but it was suggested as a desirable method of approach.

It seems to make a lot of sense to me and I should have thought that the experience in Iraq would have been a convincing argument for that point of view.

Our strongest recommendations were that identification of the fact that seed was treated should be made very clear to the people who would use it. Also, that wherever possible the less toxic fungicides should be recommended, or methods which did not involve fungicides should be used, such as the breeding of resistant lines of cereals and the use of temperature and moisture for the elimination of seed-borne pathogens.

At about this time the use of mercury fungicides for the treatment of seeds was completely prohibited in the United States. And since then, this restriction has also been accepted in most other countries. The probability of this particular type of incident occurring again is, hopefully, very limited. Fungicides other than the mercuries may be involved in similar types of poisoning in cases where the treated seeds are consumed. Precautions certainly are necessary to avoid this type of incident.

Well, it seems like that was a very important trip and a very important discussion to have taken part in. Some of these seed treatments that you worked with in the early years had highly toxic qualities to them. Was there any cooperative research with public health departments then, or were those problems considered in those days?

They were certainly considered very much less than they have been in recent times. When I first started in with seed treatments, the mercury seed treatments were very commonly used for cereal crops and we used them for a period with sugar beet seed treatments. These materials were very toxic because they were accumulative and quite a few people working with these materials absorbed enough mercury to have some effect on their health.

There were some health problems?

A few cases were identified. Probably a lot of others were minor enough that they may have been overlooked, but this was a real problem for research workers and for the operators of the seed treating equipment. As the attention was focused much more on health related problems, most of these highly toxic materials have been discontinued. Mercuries are no longer used in the treating of any seed crop. And arsenic compounds have been eliminated. We are now working with materials that are much less toxic and much less hazardous to the health of the people investigating them or actually using them. At that time there was no development of a toxicology department, and most people just used whatever precautions were available to minimize the effects.

Today, now that Davis has a medical school, is there any cooperative research between the plant pathology and the human pathology departments at the University?

I would say there is very little contact with reference to control of plant diseases because the organisms are quite different. But in recent years a considerable amount of research information that has been developed in the areas of human medicine and veterinary medicine is very closely related to work that is now going on with plant diseases. The mycoplasmas, for example, were first studied and understood in the fields of medicine and then very similar types of organisms were found to cause certain plant diseases. There has been a great deal of research in this area in the last ten or fifteen years.

Well, getting back to your post-retirement activities, do we have any other trips or incidences to talk about?

OECD GRADUATE COURSE AT ZARAGOZA, SPAIN, 1975

Well, on the next trip you and I were also involved. Both of us were invited by Dr. Raymond Hull to teach in a graduate program which was organized by the OECD [Organization for Economic Cooperation and Development], headquartered in Paris. This organization had established a graduate program whereby young people would be brought from countries all over southern Europe into the Mediterranean Institute, located a few miles outside of Zaragoza in Spain. I was invited to participate in the two week course which would be concentrated on the identification and control of diseases affecting sugar beets. The full course was for a period of about five months, on all aspects of sugar beet production. For the topics relating to diseases, insects, irrigation, and a few other aspects, the OECD asked Raymond Hull to organize the material and to select the instructors for these sections of the course.

You've had a long association with Raymond Hull. What was his position at that time?

He was the director of the Brooms Barn experiment station near Bury St. Edmunds, in England. This was a branch of the Rothamsted station and specialized on problems relating to sugar beet production.

How many students were in the classes that you taught?

There were about twenty-five, and they were from a number of European countries, particularly Spain, Portugal, Greece, West Germany, Holland and Sweden.

This was in 1975, the year just after your trip to Brussels and Geneva.

I remember that you were there just before we were and that you participated in a similar program.

Yes. Skip and I were there in early spring of 1975, I think in March or April.

I was there in April and early May.

I remember, that was a very interesting experience for me and I'm sure it was for you, too. How did you get along with the two weeks course there?

Well, I would say that it worked fairly well. We had a very suitable lecture room, projection equipment and so forth, but the greatest handicap was our shortage of fresh materials or teaching supplies that could be used to show the students exactly what we were talking about. As a result much of the instruction had to be oral and illustrated by colored slides. It was too early in the spring to be able to collect many of the typical sugar beet diseases and they did not have a laboratory or greenhouse facilities where these diseases could be developed in time to use within the program.

Jane accompanied you on this trip.

Yes, she did.

Where did you folks go after this meeting, did you come straight home or did you travel around Spain?

Just before the course I rented a car and we made a trip from Zaragoza down to Madrid and to the southern part of Spain around Sevilla and Malaga in an attempt to collect as much material as possible that could be used in the course, but this was only partially successful. After the course, we took a short visit to Andorra, a very small independent principality located in the mountains between Spain and France, and then we proceeded to Barcelona and flew to London and there rented a car to spend about a week, touring in Wales, and areas of the British Isles that we had not previously visited.

It seems to me that you have covered the European continent pretty thoroughly. Where haven't you been?

Well, it is true that we have travelled pretty extensively in Europe and in South America, but have visited Greece only briefly and have not been able to visit Russia or in areas around the southeastern Mediterranean.

Well, what's holding you back, you ought to make that trip.

That may be next.

FIRST MEETING OF LATIN AMERICAN SUGAR BEET TECHNOLOGISTS, URUGUAY, 1976

Lysle, as I recall, in November or early December of 1976, there was a meeting in Uruguay at the same location that you had visited years before, at the hosteria, Bella Vista near Maldonado. This meeting was the first Latin American meeting of sugar beet technologists and there were two North Americans invited, you and I. We attended that meeting and I know you gave one talk *that you were prepared for and you were asked to give another talk, I think on curly top, kind of extemporaneous. For me that was a very interesting meeting, too. Do you have any reflections on those meetings?*

Well, we also enjoyed this trip. It was somewhat of a surprise when we received the invitation because I think only three invited speakers from outside Uruguay were asked to come to this meeting. One from Germany, you and I.

I had been in Uruguay on three previous occasions, as I've mentioned, but this trip gave me an opportunity to meet with all of the people from Uruguay and Chile who were involved in leading positions in investigations of problems affecting the sugar beet industry.

Let's see, didn't you talk about Sclerotium rolfsii during those meetings?

Yes, that was the main topic that they asked me to discuss. I also talked about curly top and some other diseases and Gaston Gonzalez from Chile discussed seedling diseases, including the work that they had done with *caida* which I've previously mentioned.

Following that meeting I remember they took us on a bus tour of some of the sugar beet growing areas of Uruguay. We attended asados *in various places that are supposedly typical of the Uruguayan gaucho feasts. I remember the one that they called "asado incrudo," I think, where they have the slab of beef with the hide still on, on slanted grills up against a fire for cooking. The gauchos were standing around with their knives in their sashes behind their backs. It was all very colorful and the food was excellent.*

Yes, some of those experiences will never be forgotten. I remember the particular *asado* at Montes, where they brought out a typical Uruguayan horse and I think you took this horse for a ride.

Those gauchos were there as instructors and didn't let us do anything wrong. They let anybody who was brave enough try to ride one. I think that's the last horse I've ridden.

This tour by bus went from Bella Vista to La Sierra and then to a factory district at Montes where we observed a number of field experiments. Then we proceeded west to Paysandu and Mercedes, the locations of two other sugar factories. We also visited field experiments and were wonderfully entertained there.

At one of these luncheons I suddenly discovered that I was on Channel 3 of television. An interviewer was asking me a number of questions about production of sugar beets in Uruguay and about the conference that was being held. Of course, I required a translator and this had to be done extemporaneously.

I remember I had to leave shortly after that conference. When I got on a plane from Montevideo to head back to the States, and I happened to sit next to a lady who told me, "Oh, I saw you on television." I had been standing around while the interview had been going on too. So, instant celebrities! (Laughter)

ONGOING RESEARCH PROJECTS, PUBLICATIONS

You and I both know your retirement years have been productive ones, but at the time did you have any reservations about retiring when you had had such an active academic career?

No, not really. I was asked by Professor James Meyer, then dean of agriculture, whether I wished to be reappointed in the department. It was possible for a certain number of people to be reappointed, usually on a part-time basis, to continue their activities. I told him that I intended to continue some of the activities, but I thought it would be better for the department if they were able to hire a younger man to replace me on a full-time basis. If space could be provided, I would continue some aspects of my research activities.

Hasn't the University made it possible for you to do that on a continuing basis? You seem to have remained very active.

As far as space is concerned, at first they provided me a nice office and a laboratory, but as time went on, things became a little more crowded and I was asked to share an office and I lost my laboratory. Still later, four of us—now three of us—are placed in a very small office where I have desk space and I was able to borrow a certain amount of laboratory space from Dr. Dennis Hall, who is extension plant pathologist. I have a small amount of space in the greenhouse and I have had funds from the California Sugarbeet Research Committee, which includes both the growers and the processors. They have provided enough funds that I can hire an undergraduate student who helps me with some of the experimental work. So I have continued activities particularly with two aspects. One is working with you [Jack Hills] on the use of systemic fungicides for control of powdery mildew on sugar beet, and the other project is on the evaluation of new experimental fungicides for seed treatment.

Lysle, your impressive bibliography, extending over a fifty-year period, lists 116 publications of various types. Could you explain the distinction between technical and popular publications?

The technical publications would be the direct reports of the experimental work,whether it was in the laboratory or in field plots. The popular articles were a summation from the experimental studies of information that would be directly useful to the growers of certain crops or for the agricultural industries.

Your most recent one listed was in 1979. Do you plan additional research publications?

There will be very few additional publications. I probably will put out one in the near future on the evaluation of experimental fungicides for seed treatment. Some of the work that's still being carried out on the control of powdery mildew sugar beets by the use of systemic fungicides will also lead to some types of publication.

Okay, Lysle, if you have nothing you'd like to add to this account of your research work, I'll let Mr. Dickman conclude these interviews.

· XXI ·

FAMILY AND COMMUNITY ACTIVITIES

The interviewer is A. I. Dickman.

Following your discussions with Jack Hills about your university work, let's go back in time a little bit and talk about your family and your interests away from the campus. You've mentioned your three children, Joann, Douglas and Janet. What sort of recreational activities or travelling did your family enjoy? With your busy schedule, did you ever take a vacation?

Yes, we found time for vacations. In the '30s and '40s most of these revolved around trips to the Sierras, very often with friends of ours who had cabins in the mountains. And very often these would involve a pack trip and lots of fishing.

You took pack trips into the high country?

Yes, primarily in the Sierras. Periodically we would have summer vacations along the coast. One of our favorite areas was Carmel. For a number of years we went to Carmel and had our vacations in cottages near the beach. Shortly before my retirement in 1968, we purchased a cottage at La Selva Beach, located on Monterey Bay between Santa Cruz and Watsonville.

What about winter sports? Didn't you do some skiing at an early point in time?

Yes, in the winter we often went on ski trips where we would stay with the Woody Wilsons in their cabin at Strawberry on Highway 50 and then our two families would engage in skiing.

What year did you start skiing?

It was around, I think, 1934. From that time on we went on several ski trips almost every year. A rope tow or two [on I-80] was the most modern equipment that you could expect to find, and very often we just had to climb the hills and then ski down.

What kind of equipment did you have in those days?

Well, at first we all had pine skis and bamboo poles, then hardwood skis later on. None of us were very proficient at it, but we had a lot of fun trying. Later our children skied much more extensively and had better equipment. They became quite proficient skiers, but Jane and I were primarily snow bunnies.

Your two older children were teenagers when your youngest daughter was born during World War II. Did your family activities change at that point?

Well, only slightly. Our two older children were in high school in 1945 and, of course, the younger child did create some changes in the family. But Janet went along with us at quite an early age on our summer trips and other activities. We took her to the mountains when she was four weeks old and she tried skiing when she was under three years of age, so I wouldn't say that it made any great change in our way of life.

You've already talked some about the Leach family background. Is there anything you would care to add?

From what I've been able to learn in recent years, my paternal great-grandfather, Peter Leach, was born in Devonshire, England in 1799, and in 1819 he married Charity Taunton who was born in 1802. Taunton is a well-known town in Devonshire and I assume that there's some relation between the name and the community. Peter and Charity Leach had five children: James [b. April 14, 1820], Harriet [b. February 7, 1821], Isaac [b. April 1, 1823], Jane [b. December 13, 1825], and Mary [b. May 1, 1841], all born in England.

My grandfather, Isaac Leach, married Louisa Clair, from London, who was born in 1825.

Do you know the date of their marriage?

I don't think I have that but I have been told by a cousin that they lived at Kensford, a parish of Kantisheer, Devonshire, until they emigrated to the United States in the early 1850s. As I mentioned earlier, they settled in Cuyahoga County near Chagrin Falls, Ohio, now a suburb of Cleveland. A considerable part of their farm there was a maple grove and my first recollection was of receiving maple syrup and maple sugar at Christmastime. One memento I have from the Leach family in Ohio is an engraved silver cup that my grandfather was awarded for plowing the straightest furrow at the Ohio State Fair.

In 1968 Jane and I visited Ohio and found that although the old farm is no longer owned by anyone in our family, it is still called "Leach Hill." But the farm is now bisected by a secondary highway, so that the house is on one side and most of the farm is on the other side of this highway.

Who were Louisa and Isaac Leach's other children?

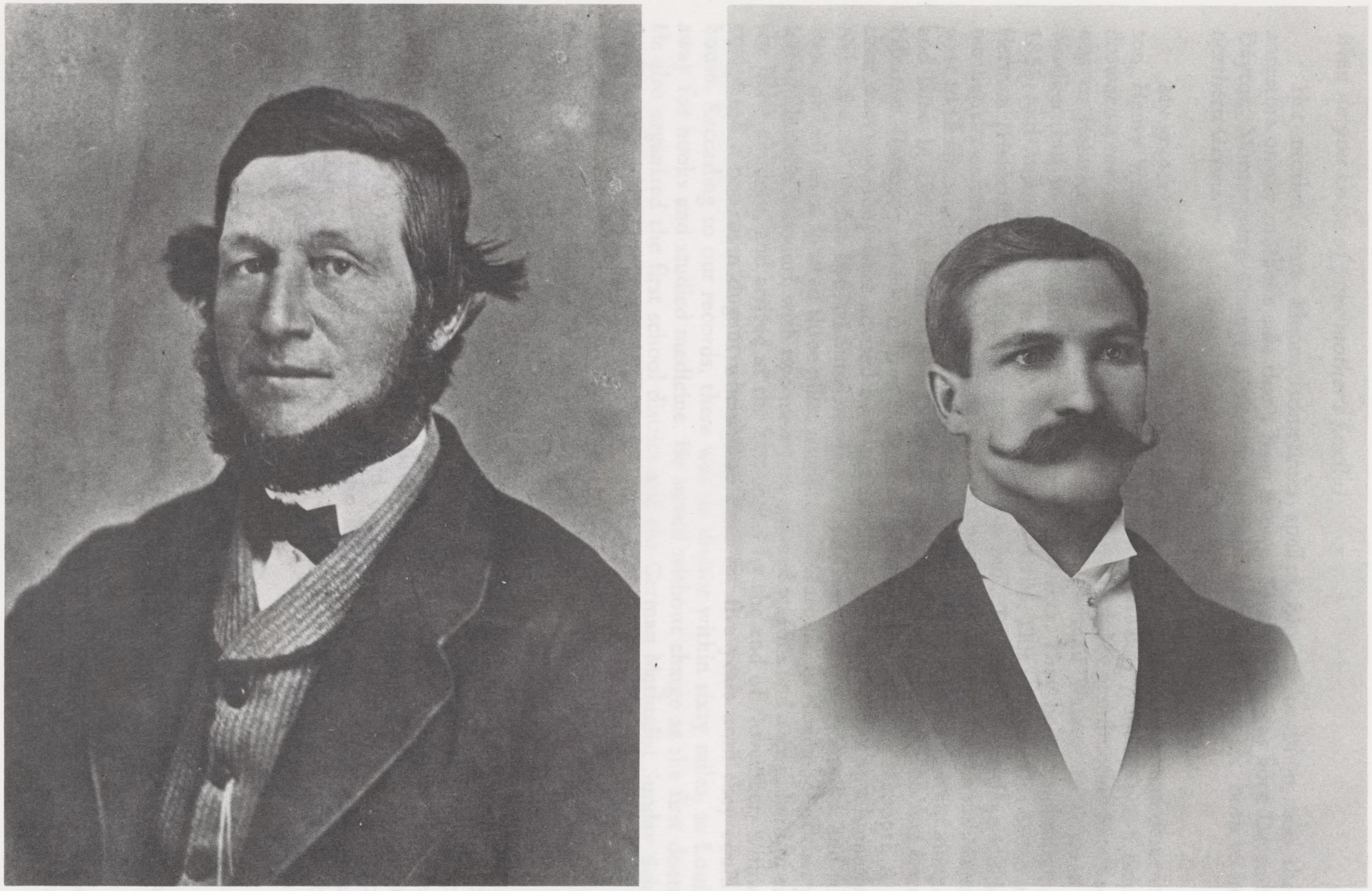
(Glancing at notes). I don't have all their birth and death dates. William became a cobbler in Cleveland. Henry remained at Chagrin Falls and had one daughter, Villa, who married Mr. Osborn. They had a daughter, Christine (Dougherty) who is now living in Chagrin Falls and a son, Roland Osborn, now living in Burton, Ohio.

Thomas [1858-1925], stayed on the homestead near Chagrin Falls. He married May [1864-1934] and had one daughter, Margaret [Hine], who had no children.

John Wesley, my father, was born at Chagrin Falls on March 31, 1861 and died at Arkansas City, Kansas on September 23, 1933 in an auto accident.

Fred, the youngest, was born August 23, 1870 and died December 31, 1951. He had an insurance agency in Chagrin Falls. His wife, Jennie, was born May 15, 1875 and died March 13, 1951. She was an invalid for many years as the result of a stroke. They had no children.

Isaac Leach died April 6, 1885 and Louisa died April 23, 1900, so I had no opportunity to know my Leach grandparents. Both Uncle Henry and Uncle Fred visited us in Fremont, about 1910,and I visited Uncle Fred and Aunt Jennie in 1946. My wife Jane and I also visited my cousin, Villa Osborn and her daughter at Chagrin Falls in 1968.



Left: Isaac Leach (grandfather). April 1, 1823–April 6, 1885. (Date of photograph unknown). *Right:* John Wesley Leach (father) at age thirty-three in 1894. March 31, 1861–September 23, 1933.

What do you know of your mother's family?

Her mother, Sara Bloom, sometimes spelled Bloume or Blum, was the oldest of the ancestors on my mother's side that I have known. Sara Bloom was born December 15, 1837 at Dayton, Montgomery County, Ohio and was the fourth child of six, born to immigrants from northern Germany.

My grandmother was first married to Wilhelm Wessel, also of German descent. They had one son, Henry William, born October 26, 1853. Her husband, Wilhelm, was a fisherman and was drowned a few months after the birth of their only child. My grandmother Sara's second marriage was to Heinrich Wilhelm Walker, also known as Herman Walker. In German the name was Woelker, but in this country it was changed to Walker. They had two children: Mary, born in 1858; and Andrew, born in 1859, at Cincinnati, Ohio. This marriage also ended in tragedy when Herman [Heinrich] died after being gored by a bull. To support her three children, my grandmother thereafter worked for a family by the name of Miller and when the Miller family moved west to Chillicothe, Livingston County, Missouri, Sara and her children went along. There she met and later, in 1861, married Louis Drewing, anglicized von Draving, who was born [in 1833] in Hanover, Germany, and later lived in Bremen before immigrating to Missouri. A few years after their marriage, they were advised by a circuit riding minister to take up a homestead in southern Nebraska. In August 1867, Louis Drewing visited Gage County, Nebraska, and filed on a homestead of 160 acres, located three miles northeast of Clatonia. In November of that year the family left Chillicothe, Missouri, with two ox-drawn covered wagons and a small herd of cattle, driven by the older children, and arrived at the homestead by the end of November. Within a week, they completed a one-room dugout where they lived for five years until they could build a frame house. According to our records, there was no doctor within sixty miles, so Louis Drewing sent away for books and studied medicine. He served without charge as the first doctor at Clatonia. He also organized the first school district and the German Methodist Sunday School, serving as its first superintendent.

How many children did the Drewings have?

Louis and Sara had eight children. Three of them were born at Chillicothe, Missouri and five at Clatonia, Nebraska. Three of the children died before six years of age of diptheria or pneumonia, and Lewis Drewing himself died in 1884 of pneumonia at fifty-one years of age. One of the stories that has been handed down about Louis Drewing is to the effect that among German families at that time it was believed that if there was a serious illness in a home, the first person who visited that home would contract the illness, but later visitors would not contract the disease. So Louis Drewing, who had been a sickly child, was encouraged to visit this home where there was a serious illness and it was arranged that his older brother would visit this home afterward. But Louis hid out until he saw his brother go into the house and then he came second to visit the home. (Laughter)

What about your mother's older brothers and sisters?

Their names were Albert, Edward, Jennie (Geisert), John D. (also known as Daniel), Arthur, Webster, and Ester.

In 1890, after the death of Louis Drewing, my grandmother moved to Lincoln, Nebraska with her children. She remained there until 1903, when she returned to Clatonia where she lived until her death on May 1, 1926, at the age of ninety years and five months.

We visited Clatonia a number of times in the later years and I was present during the last days before my grandmother's death.

How large a town was Clatonia, where your mother [Emma Drewing Leach] had grown up?

Well, it was a very small country town. I would guess that it was less than 500 population—only a few places of business, a church, a school and a group of houses, because the town depended very largely on the farming community around it.

Did the Drewings live in town or on a farm?

They lived on a farm during their earlier years, but as my grandmother advanced in age, she did have a home in Clatonia, which is located about thirty miles south of Lincoln. During most of my youth we lived at Fremont, some fifty miles north of Lincoln.

Are any of the family members still living on that farm?

I'm not sure because I don't know who acquired the farming property. I know several of my cousins moved to Lincoln and to other areas some distance away.

During visits with your grandmother did you ever have conversations about her own early life?

Well, we had many conversations, but I don't remember that she included many of her recollections of her earlier life. Most of what I know about that was the result of compilations that were made by second cousins who are still living near Clatonia and near Lincoln.

So someone has done some family research?

Yes. These two ladies, Myrna Walker and Lucile Hallman, have gone to considerable effort to bring together some of the early records, and they are planning to write a family history.

What about your wife's side of the family? Where had her parents come from and what did they do?

As I've already indicated, her grandparents on her mother's side came from Northern Ireland and were of Scotch Presbyterian stock. Jane doesn't know anything about the Conn family background—only that her grandfather, Hugh Conn [1846-April 5, 1928], came to the United States some time in the 1870s and settled in Boone, Iowa, then located at the end of the railroad line. Supposedly, he came with two brothers who went on to California, but they lost all contact with each other after that. Hugh Conn's bride-to-be, Sara Jane MacMechan [1850-January 29, 1935], traveled alone from Ireland to join him in Iowa where they were married. (It was her family home we visited in Ireland some years ago.) One recollection she had about her journey was of passing through Chicago just after the Great Fire [October 8, 1871].

I gather that Jane's grandfather Conn had a very successful feed and fuel business. He sold coal for fuel and hay and feed for horses and livestock. The Conn's three children were David, Grace and Margaret, Jane's mother. Margaret, who was born in 1875, married Dr. Charles [Amzi] Rhoads who grew up in Boone also and later went to dental school in Chicago (now the University of Chicago). After their marriage in 1902, they also made their home in Boone. Jane [b. February 4, 1906] and her sister Betty [Elizabeth] were born there and went through the grades and high school until they both went away to college at Iowa State College, only twelve miles from Boone. A younger sister, Catherine, died at the age of eight following the flu epidemic in 1919. Betty married Chick [Chester A.] Lee and still lives in Ames where Chick was an expert in poultry diseases in the School of Veterinary Medicine.

Where did the Rhoads family originate?

(Looking at notes.) Jane doesn't know for sure, but family Bibles and census records show that her grandfather, Gideon Secor Rhoads, was born at Elbridge [Onondando County], New York on March 12, 1830. His parents were Anthony [1780-1868] and Betsy Secor [1786-1863] Rhoads. Gideon Rhoads married Ann Elizabeth Zimmerman [b. May 13, 1838], who died in Boone on January 17, 1877 when Jane's father was only two years old. There were two older children: Mary who was born in 1867 while the family was living in Illinois, and John who was born in Iowa in 1872. A younger daughter, Mable, was born in 1881 following a second marriage to Ruby Mayo. Gideon Rhoads was a cabinet maker and served for many years as the city marshall of Boone. He was a veteran of the Civil War, having served with Company C, 77th Illinois Infantry. Some of the New York relatives once indicated to Jane that the Rhoads family may have first settled in Massachusetts before the Revolutionary War and later in New York, but that is not certain.

Did your children have a chance to know some of their grandparents?

Yes, especially their grandmothers. Jane's father died as a result of cancer [in 1946]. Her mother then moved to Davis, rented a small house not far from our own and lived there for many years. She was very active in writing children's stories and also wrote for some of the local newspapers, and made many friends in Davis until her death, which occurred while she was visiting at Ames, Iowa in October 1962.

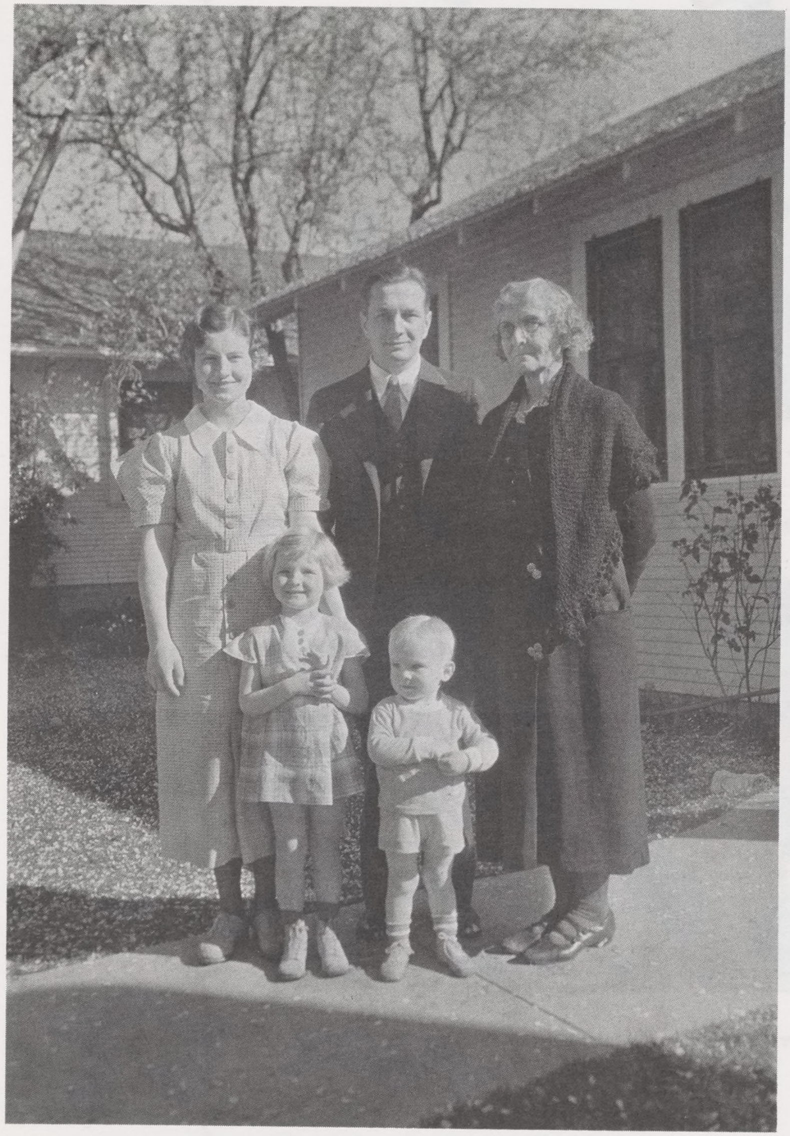
My mother had a similar experience. My father was killed in an auto accident [1933] and some time after that my mother moved first to Seattle where my older brother lives, and later she came to Davis and rented a small cottage and lived here as long as she could live independently. She died at Woodland, California on August 29, 1959.

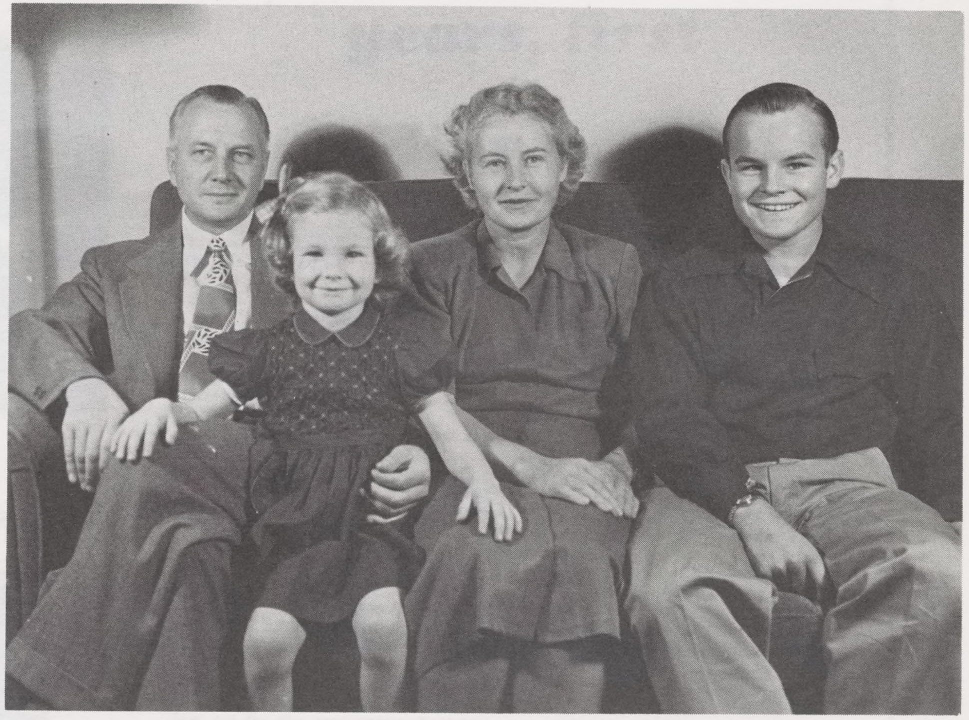
What about the education of your children? Did they attend school on the Davis campus?

Two of them started college on the Davis campus. Our oldest daughter, Joann, attended for one year and then transferred to the University of California at Berkeley, where she graduated in 1951 with a B.A. degree in Decorative Arts. In 1952 Joann married Richard J. Larkey, the son of Dr. and Mrs. Jefferson Larkey of Orinda. Richard Larkey received his B.A. degree at UC Berkeley and his M.D. degree from McGill University, Montreal. They lived in Germany from 1959 to 1961 while Dick served in the U.S. Army. Since 1962 they have lived in Davis or near Winters while Dr. Larkey conducts his family practice in Davis.

They have two children, Diane, born December 20, 1953, in Montreal and Jefferson Douglas, born March 4, 1955, also in Canada. Their daughter graduated from Colorado State University in 1972 and was married to Greg Larson, formerly of Greeley, Colorado. They now live in Roseville, California. Jeff Larkey attended Santa Rosa Community College and Cabrillo College near Santa Cruz [Ca.], where he now lives.

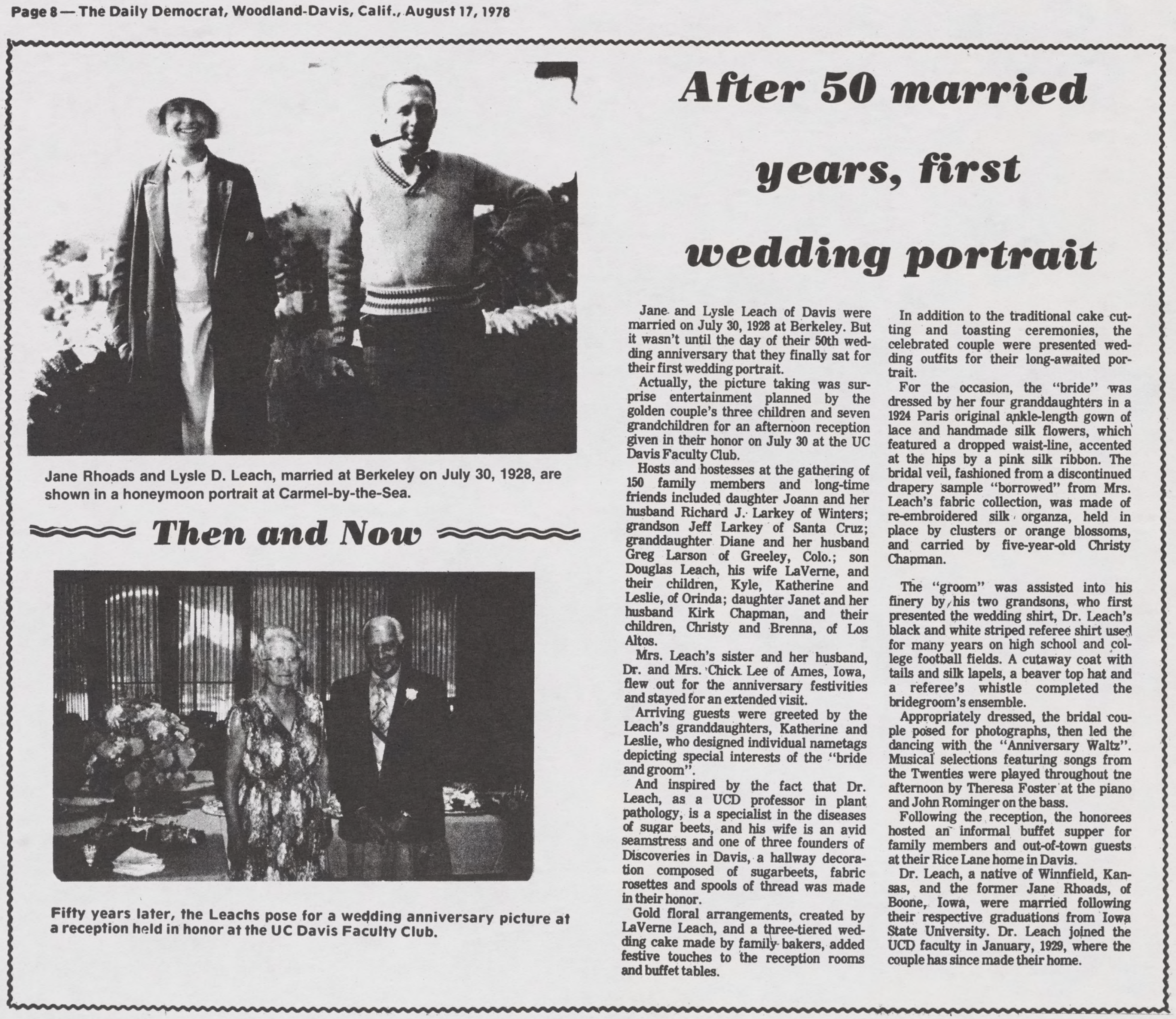
Douglas, our son, after finishing Davis high school, also attended the Davis campus his first year and then transferred to Berkeley where he received his undergraduate degrees in business administration and industrial engineering in 1957. In 1959 he married LaVerne Rohwer, the daughter of Ray and Elda Rohwer of Dixon. Both the Rohwer and the Schmeiser (Elda's parents) families were pioneer families of the Dixon area. Douglas was employed as an engineer by the Foremost Dairies in San Francisco and later by the Cutter Laboratories in Berkeley. Recently he accepted a position with Syntex, a pharmaceutical company headquartered in Palo Alto, California. Their home is in Orinda.





Above: Lysle and Jane Leach, with Janet and Douglas (1951).

Left: Lysle and Jane Leach, with children Joann and Douglas, and Lysle's mother Emma Drewing Leach (1934).



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Jane Rhoads and Lysle D. Leach, married at Berkeley on July 30, 1928, are shown in a honeymoon portrait at Carmel-by-the-Sea.

Then and Now

Fifty years later, the Leachs pose for a wedding anniversary picture at a reception held in honor at the UC Davis Faculty Club.

After 50 married years, first wedding portrait

Jane and Lysle Leach of Davis were married on July 30, 1928 at Berkeley. But it wasn't until the day of their 50th wedding anniversary that they finally sat for their first wedding portrait.

Actually, the picture taking was surprise entertainment planned by the golden couple's three children and seven grandchildren for an afternoon reception given in their honor on July 30 at the UC Davis Faculty Club.

Hosts and hostesses at the gathering of 150 family members and long-time friends included daughter Joann and her husband Richard J. Larkey of Winters; grandson Jeff Larkey of Santa Cruz; granddaughter Diane and her husband Greg Larson of Greeley, Colo.; son Douglas Leach, his wife LaVerne, and their children, Kyle, Katherine and Leslie, of Orinda; daughter Janet and her husband Kirk Chapman, and their children, Christy and Brenna, of Los Altos.

Mrs. Leach's sister and her husband, Dr. and Mrs. Chick Lee of Ames, Iowa, flew out for the anniversary festivities and stayed for an extended visit.

Arriving guests were greeted by the Leach's granddaughters, Katherine and Leslie, who designed individual nametags depicting special interests of the "bride and groom".

And inspired by the fact that Dr. Leach, as a UCD professor in plant pathology, is a specialist in the diseases of sugar beets, and his wife is an avid seamstress and one of three founders of Discoveries in Davis, a hallway decoration composed of sugarbeets, fabric rosettes and spools of thread was made in their honor.

Gold floral arrangements, created by LaVerne Leach, and a three-tiered wedding cake made by family bakers, added festive touches to the reception rooms and buffet tables.

In addition to the traditional cake cutting and toasting ceremonies, the celebrated couple were presented wedding outfits for their long-awaited portrait.

For the occasion, the "bride" was dressed by her four granddaughters in a 1924 Paris original ankle-length gown of lace and handmade silk flowers, which featured a dropped waist-line, accented at the hips by a pink silk ribbon. The bridal veil, fashioned from a discontinued drapery sample "borrowed" from Mrs. Leach's fabric collection, was made of re-embroidered silk organza, held in place by clusters or orange blossoms, and carried by five-year-old Christy Chapman.

The "groom" was assisted into his finery by his two grandsons, who first presented the wedding shirt, Dr. Leach's black and white striped referee shirt used for many years on high school and college football fields. A cutaway coat with tails and silk lapels, a beaver top hat and a referee's whistle completed the bridegroom's ensemble.

Appropriately dressed, the bridal couple posed for photographs, then led the dancing with the "Anniversary Waltz". Musical selections featuring songs from the Twenties were played throughout tne afternoon by Theresa Foster at the piano and John Rominger on the bass.

Following the reception, the honorees hosted an informal buffet supper for family members and out-of-town guests at their Rice Lane home in Davis.

Dr. Leach, a native of Winnfield, Kansas, and the former Jane Rhoads, of Boone, Iowa, were married following their respective graduations from Iowa State University. Dr. Leach joined the UCD faculty in January, 1929, where the couple has since made their home.

Fiftieth wedding anniversary, July 30, 1978 *(Davis Daily Democrat, Woodland-Davis, California, August 17, 1978).*

Douglas and LaVerne have three children. Their oldest son, Kyle, born June 21, 1960, is now attending California State University, Humboldt. Their older daughter, Katherine, born on October 16, 1961, is attending California Polytechnical University (Cal Poly) at San Luis Obispo where she will graduate in June 1983, and the younger daughter Leslie, born November 6, 1964, is on an assignment with the American Field Service for a year's experience in Venezuela. She has been admitted to the University of California at Davis and will attend here in the fall of 1983.

Our youngest daughter, Janet, did not attend the University at Davis but went from high school to Oregon State University at Corvallis, Oregon. She attended two years before she [in 1965] married one of her classmates, Kirk Chapman, a native of Corvallis who upon his graduation as a civil engineer entered the Air Force. After their marriage they lived in Texas for a period of time and then were transferred to Germany where Kirk was stationed at Ramstein Air Base for four years as a pilot of a high-speed reconnaissance plane. After his discharge from the Air Force, they moved to California where Kirk accepted a position in the engineering department of Coen Manufacturing Company near Burlingame [Ca.]. Janet and Kirk are now living at Los Altos with their two little girls, Christy, born September 22, 1973, and Brenna, born March 7, 1978.

So you have seven grandchildren? Haven't you taken some of them with you on your travels in recent years?

Yes, we have. When she was sixteen, we arranged for Diane to meet us in Europe. She flew first to Germany where she visited with Janet and Kirk, and then to London. We were already in Ireland at that time. Jane flew across to London to meet her and they toured in England for a few days and then came back to Ireland, and she was with us there for more than a week. During that time we traveled to the west of Ireland and I guess one of the highlights would be when we lived at Ashford Castle, which is a beautiful old castle now converted to a hotel. It is located just about six miles north of Galway. While we were there, Diane and I took the boat from Galway to the Aran Islands, and rented bicycles and toured a good portion of the larger of the three Aran Islands, returning in the afternoon by boat just in time for dinner at Ashford Castle.

That sounds like a pleasant day.

It was. Then several years later, during a meeting of the American Phytopathological Society in Mexico City, we arranged for three of the grandchildren, Jeff Larkey and Kyle and Katherine Leach to be there with us. We included the Ballet Folklorica at the Beauxes Artes and visited a lot of the important sights around Mexico City, including Chapultepec Castle, and the pyramids.

GOLDEN WEDDING ANNIVERSARY, 1978

You and your wife celebrated fifty years of marriage on July 30, 1978, with a gala party at the UCD Faculty Club. That must have been a pleasant occasion for your family?

Yes, this very nice occasion was arranged by our three children and their families and brought together perhaps 200 of our friends from Davis and surrounding areas. It was very pleasant and very well arranged, and we thoroughly enjoyed it and appreciated it.

While your family was growing up, you had a pretty busy schedule on campus. Did it allow time for any participation in community activities? How about activities that your children might have involved you in?

SCOUTING, WARTIME ON THE HOMEFRONT

Well, one of the early ones was the Boy Scouts. I was asked to be a commissioner and I served as an assistant to Bill Hewitt when he was in charge of a scout troop. During that time [in the 1940s] we organized the Explorer Scouts for the older group of students to keep them active in scouting after they reached the age when many would drop out. Bill Hewitt and I planned high Sierra pack trips and for four years in a row we went with the Explorer Scouts into areas near Tahoe and down near Bishop in the Mammoth Lakes area where we would camp for periods of a week or more. We would cover long distances and camp and fish during that period. This seemed to be very popular with the Explorer Scouts.

It was somewhat expensive for the scouts at that time so we thought up another activity, which was selling Christmas trees to provide funds for the annual trip. Bill Hewitt and I visited a Mr. Geiberson who owned a considerable amount of land east of Davis. He also owned some forest land in the Sierras. He gave us permission to take the Explorer Scouts up there and to cut trees. These were stored in the colder areas and then brought down to Davis about three weeks before Christmas, and the trees were sold. This project not only provided some very high quality of Christmas trees but it earned a considerable amount of money which went quite a ways toward funding the Explorer Scout trips during the summers.

The scouts are still doing that at Christmastime.

Yes. I see the trees sold every Christmas.

Was your wife involved in the Girl Scouts?

Yes, she worked with the Girl Scouts, but she'll have to tell you what she did. She was quite active and held an office in the scout organization for several years.

What activities were you involved in during the war [World War II]?

Well, I was a block warden in the civil defense operation that was activated right after Pearl Harbor. My job was to see that lights were extinguished during periods of blackout and, working with a neighbor, we built ladders and prepared to put out the fires in case we were bombed.

You were expecting a bombing attack at Davis?

They really did expect an attack in the San Francisco area because there were reports that submarines had been sighted off shore and it's quite possible that if the battle of Midway had not proved so successful, the Japanese forces might have moved into central California. Fortunately, we were spared that ordeal.

Also during World War II, there was a very strong push for Victory Gardens, as they were called. We had a meeting of faculty who might be interested in this. It was agreed that we should have an organized effort and a committee of three was selected to supervise and plan the Victory Garden program. This consisted of Oscar Lorenz from the vegetable crops department and Lloyd Doneen from irrigation. I was the third member, particularly to be responsible for pest control.

We made arrangements to take over a large area directly south of First Street on land owned by the University. This is the same site where the Aggie Villa housing was located after the war. We surveyed the area and divided it into plots consisting of about 12 rows or beds for each plot and 100 feet long. Seed beds were thrown up and we marked out and numbered the plots. Then to attract attention to this program, we had what we called a "land rush" patterned after the

Oklahoma Land Rush, where everyone who applied for a plot was encouraged to come to the area and they drew lots to find out which plot would be theirs for the Victory Garden.

Was this open to everyone in the community or just to university people?

It was started by the university people but was open to anyone in the community. The newspapers and the radio representatives from Sacramento were here and they gave us quite a little publicity about this. Then each individual, or family, was responsible for their particular plot of land. They would choose the crops to grow and do the planting. We held informational meetings where Oscar Lorenz would talk about varieties and productivity and methods of production. We arranged that irrigation water would be available, I think, one day each two weeks. The water would come down a central ditch and then would be diverted into the plots for irrigation. This turned out to be a very productive program because there was more food produced than individuals could eat. Anyone who was interested in squash or potatoes or tomatoes or corn during the season of the year found plenty of it available without any charge. This operated for a period of about three years and certainly served its purpose very effectively.

One other question about the wartime. Didn't a number of faculty families become farm laborers during the war as well to help harvest the crops?

Yes. Because of the shortage of labor during the wartime and the push for food production, quite a large number of the faculty members and their families worked on the farms nearby. Our family worked on a ranch where fruit was produced—apricots and prunes primarily. We worked there on weekends and any other time that could be spared. The men would usually work in the fields harvesting the apricots and in the dry yards for the drying of the fruit, whereas the women were in the cutting sheds where they would cut the fruit and place them on the trays. We also worked in tomato harvest later in the season when there were not enough laborers to get the tomatoes out of the field. We used to go to work at the University early in the morning and get off about 4 o'clock, and then work in the fields from about 4 until darkness, and also on weekends.

It was all hand harvesting at that time?

It was all hand harvested. We were paid regular wages for these activities but it was done primarily because of the shortage of labor.

I didn't mention that during the wartime I was also on the gasoline rationing board, which was a thankless task because almost everyone felt they were entitled to more gas than the regular coupons would provide. We had the difficult task of deciding who should receive the extra rations and who should be held to the minimum amounts.

DAVIS PLANNING COMMISSION, 1942–1950

During World War II, I was asked to be on the city planning commission. I served as a member of the Davis Planning Commission for a number of years [January 1942–August 1950] and as chairman for two or three years.

What sort of problems was the planning commission dealing with at that time?

Well, this was the time when the town was beginning to grow quite rapidly so there were additions to the town. There were plans for the development of whole new areas, such as the Oeste tract just to the east of Anderson Road which was put in at that time. It was the time when the [New Varsity] theatre was built downtown, and the first extension of buildings on Second

Street from F Street to the west all came up during this period. The additions north of Eighth Street were added to the city and we had the problem of planning the streets. One of the main efforts was the penetration of F Street from about Seventh north to Fourteenth Street. In the other part of town was the annexation of the area that was known as Elmwood and . . . .

Tell me about both Elmwood and Parkside.

The Elmwood area was first purchased by the school district as a site for the high school, but when the plans were advanced the decision was made that that area was not large enough to provide the space that would be necessary for a modern high school with athletic fields and parking, etc. So the land was resold to a private developer who arranged for the improvements—the streets and curbs—and the area was divided up into lots which were individually sold [after 1953]. Because it faced on the south side of Eighth Street, there was no street running through to Russell Boulevard, so it had to be developed as a loop road.

Parkside has a similar road.

Parkside was a loop from the north side of Eighth Street about 150 yards north.

Wasn't Parkside's development similar to that of College Park, which was subdivided much earlier in the 1920's [1924]?

Parkside was also developed [in 1948] by an association of property owners who each contracted to buy one of the lots and then arrange for the improvements. Bonds were then sold to cover the improvements of the Parkside area, as was done in College Park.

YOLO COUNTY RED CROSS

One other activity you seem to have been involved in has been the Red Cross.

Yes, this occurred after my retirement. Obviously I had more time to spare and I was approached to go on the board of directors of the Yolo County Chapter of the American Red Cross. For about nine years I served on the board and took quite an active part in some of their activities. I think the Red Cross is one of the really efficient charity organizations. They fulfill a very useful purpose and deserve the support of everyone in the community.

Were you involved in the Red Cross activity during the big Yuba City flood in 1955?

Indirectly, although at that time I was not actively associated with the Red Cross. But when the levee on the Feather River broke and Yuba City and a large farming area to the south were flooded, several thousand people were displaced and they had to find housing for them. First, a large group was brought to Woodland, but the churches were about the only facilities that were available, and this was not really satisfactory for the numbers. This was in December 1955, so the administrators on the campus offered to turn over the Memorial Union, which had recently been built on campus, the gymnasium and the old Recreation Hall for the people who were displaced by the flood. A number of us were called in to help handle the people as they arrived. The Red Cross took charge of the relief activities and I have some pictures of us wearing Red Cross bands.

Didn't a lot of Davis families share their Christmas holidays with the people that were displaced that year?

Yes, there were a number of children involved among those who came here. They arranged beds for them in the Recreation Hall and in the gymnasium which is now called Hickey

Gymnasium, but their food was served in the Memorial Union. On Christmas eve, the community all joined together to give them a large Christmas party with decorations, music and entertainment, and various gifts that were provided particularly for the children.

DISCOVERIES GIFT SHOP, 1964

Your wife's artistic talents led her into a business venture that has been a real Davis success story. Can you recall how the Discoveries gift shop got organized in 1964?

Yes, Jane along with Dorothy Briggs, who was the wife of the dean of agriculture, and Esther Wagner, wife of the admissions officer and registrar at UC Davis, decided that they should start up a gift shop in Davis. They rented a small unit downtown on E Street and started purchasing supplies for their store where the three of them shared working hours. This seemed to be a very successful venture and later they had to expand into much larger quarters and had to hire additional help to take care of the customers. This continued very successfully as an expanding business until 1968 when I retired. Then, Jane decided that she would also retire because we would be traveling more and she didn't feel that she could continue to devote the required time to the store. Esther Wagner also decided to withdraw from the partnership and she and her husband later moved to Stockton where Hans was employed as registrar by the University of the Pacific. Discoveries is a very popular gift shop for Davis and for the surrounding area. Dorothy Briggs, who took the sole ownership of Discoveries in 1968, subsequently built a two-story building with the lower floors occupied by Discoveries and the upper level for professional offices.

I can vouch for the fact that you took time to enjoy a little golf after you retired.

Yes, I took it up and worked at it pretty hard for a while, but in recent years I haven't found enough time for it.

You had never played previous to 1968?

No, I never played until I retired and it came fairly slow at that age.

But we did have good times on the course.

Yes, I really enjoyed playing golf with you, George Marsh, Roy Bainer, Harold Reiber, Jim Douglas and my other retired friends.

XXII

RETIREMENT, SPECIAL HONORS

RETIREMENT DINNERS, TREE DEDICATION

You've mentioned your retirement. I understand that when you became professor emeritus in 1968, you had not one, but two retirement dinners to attend.

Actually, there were three. That year the University held a joint retirement on April 3, 1968 in Freeborn Hall for all of the faculty members in the College of Agriculture and Environmental Sciences who were retiring in that particular year. Later two other groups also arranged retirement parties. The first party was held at El Macero Country Club and was arranged by the California Beet Growers Association and the Cotton Growers Association.

This was especially in your honor?

That's right. I had worked with these two groups quite extensively and they showed their appreciation by arranging a very pleasant evening. J. Earl Coke, formerly vice president of Spreckels Sugar Company, undersecretary of Agriculture, Washington, D.C., statewide director of agricultural extension at Berkeley and vice president of the Bank of America, served as master of ceremonies. A large number of my friends from the beet and cotton industries particularly attended—some of them from considerable distances.

A week or two later the Departments of Plant Pathology at Davis and at Berkeley held a joint retirement party for Dr. E. E. Wilson and for me. This was also held at the El Macero County Club.

So the honors were done up right?

Yes, we were well retired.

You also had a tree dedicated in your honor at the time you retired.

Yes, the Horticultural Forum had a program where each year they would plant a tree on campus and dedicate it to one of the faculty members who was retiring in that year. I was fortunate enough to be the one who received a tree in 1968. They planted a rather unusual variety of eucalyptus which is still growing in front of Hutchison Hall. It appears that this particular species was not very well adapted to California, so it's having a rather difficult time.

Do you think it has plant diseases?

No, not that. It just doesn't like the climate. The question is which one of us is going to outsurvive the other.

You've received a number of honors over the years. I think you have made a list there. Maybe you would like to go over some of those and explain about them and perhaps you would discuss the ones that have meant the most to you.

The first of what I would consider as an honor was in 1951 when I was invited to Denmark and Ireland under the Marshall Plan.

FACULTY RESEARCH LECTURER, 1955

Then in 1955 I received what was considered to be the highest honor that faculty members conducting research can receive. In that year I was elected as the Faculty Research Lecturer—the thirteenth in the series of such lecturers.

Wasn't the Sigma Xi Club instrumental in establishing these annual lectures?

Yes. Before a separate Academic Senate was organized at Davis the Sigma Xi Club decided to inaugurate Faculty Research Lectures. Professor Katherine Esau was president and I was secretary-treasurer. We were authorized to appoint a selection committee and arrange the first lecture. Professor Fred Briggs was the first Lecturer selected. After the third lecture the Davis Academic Senate was organized and they took over the program.

Those accorded this high academic honor by their colleagues are asked to give a public lecture?

I spoke on my work particularly with diseases caused by soil-borne pathogens which included primarily the seedling diseases and root rot diseases affecting field crops.

I have previously mentioned that I was elected Miembro Academico by the faculty of the College of Agriculture of the University of Concepcion, Chile. This was the first time anyone had been elected Miembro Academico in the College of Agriculture and the first time that this honor had been given to a foreign visitor.

In 1965 a group of thirty-five plant pathologists were designated as Fellows in the American Phytopathological Society. I was fortunate to be selected as one of those. Since the membership of the society at that time was about 3,000, this was considered quite an honor to be included in this first group.

Did you as a group participate in some high-level research or was this an individual honor?

This was intended to cite those individuals who had made the greatest contribution to the field of phytopathology, overall. As a result of these activities, I was first listed in *Who's Who in America* in 1965.

SUGAR BEET INDUSTRY HONORS, 1974–1981

Then, in 1974 I was invited to give a special paper at the Brussels meeting of the IRRB, which is the European equivalent of the American Society of Sugarbeet Technologists.

In 1976 I was elected an honorary life member of the American Society of Sugarbeet Technologists.

And recently [in 1981] I was awarded the Silver Sugar Beet by the California Beet Growers Association.

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CLARKSBURG

February 16, 1978

Dr. Lysle D. Leach, Plant Pathologist
University of California
Room 5206 Storer Hall
Davis, California 95616

Dear Lysle:

Enclosed is a Resolution passed by our Membership at their Annual Meeting on Friday, February 10, which gives you some measure of the high regard all of us in the sugar beet industry have for you and your outstanding work, ever since you have been with the University.

I add to this, my own personal high regard for you as an individual and your deep commitment to research and continued interest in sugar beets.

Best personal regards.

Sincerely yours,

Malcolm

Malcolm Young
Executive Manager

MY:al
Enclosure

RESOLUTION

WHEREAS, agricultural research has contributed to increased food production, thereby providing food and fibre to consumers in the State of California, and throughout the United States, at reasonable and realistic prices, and

WHEREAS, from time to time researchers are called upon to help solve specific production problems as they relate to sugar beet production, and

WHEREAS, Lysle Leach has responded to the needs of the sugar beet industry in solving specific problems and introducing new ideas and techniques for increased sugar production, and

WHEREAS, although he is officially retired from the University of California, Dr. Lysle Leach continues to dedicate his energy and talents toward continued research efforts on behalf of California sugar beet growers.

THEREFORE BE IT RESOLVED that the officers, Directors and Membership of the California Beet Growers Association, Ltd., are grateful of the advancements Dr. Leach has contributed toward the welfare of sugar beet growers in California, and

BE IT FURTHER RESOLVED that the officers, Directors and Members of the Association are deeply appreciative of his continued efforts on their behalf and this resolution shall be spread upon the official Minutes of the 47th Annual Meeting of the Association and a copy be transmitted to Dr. Leach.

I hereby certify that the foregoing Resolution was unanimously adopted by the Membership of the California Beet Growers Association, Ltd., at its Annual Meeting in Monterey, California on February 10, 1978.

JUNE 6, 1934

Secretary-Treasurer
California Beet Growers Association, Ltd.

They presented you with a silver sugar beet?

Yes, this was a very attractive plaque embossed with a map of California, with a silver sugar beet superimposed diagonally across the map. This was only the second time that this award had been made in the fifty years that the association had existed.

Who was the other recipient?

That was Dr. John McFarland who was employed by the U.S. Department of Agriculture and who was largely responsible for the production of the modern varieties of sugar beets that are used in California and other western states.

I note that the plaque reads: "Presented to Dr. Lysle D. Leach February 13, 1981 for his outstanding contributions to the welfare of sugar beet growers."

POSTSCRIPT: DEDICATION OF LYSLE LEACH HALL, 1982

Then in October 1982, the Regents of the University of California, upon the recommendation of Chancellor James Meyer, approved of the naming of the graduate residence complex as the Lysle Leach Hall. This dedication occurred at a luncheon in Freeborn Hall on October 22, 1982. A portrait was unveiled at that time which is now hanging in one of the recreation rooms of the complex.

That was a high honor. Do you have some reflections on the dedication and the facility that it involved?

It was a very impressive program with speakers including Chancellor Meyer, Vice Chancellor Tom [Thomas B.] Dutton, Bill Weir who had succeeded me as the second dean of students, and Jack Oswald, one of our former plant pathology graduate students who is now the president of Pennsylvania State University. The luncheon was followed by a trip to the graduate housing complex where all the guests were shown the facilities that had just been dedicated.

That was a memorable occasion. Would you consider this one of the more important honors that you've received?

Yes, I would consider that as one of the three or four highest honors that I've had—the Faculty Research Lectureship was certainly an honor that came quite early in my career—and the dedication of the graduate residence facility as the Lysle Leach Hall would probably be the climax of a long career.

Do you have any other thoughts that you would like to include before we terminate these interviews?

It seems to me that we have discussed almost every topic that I can think of—and in fact a lot of them that I had some difficulty recalling. At this time I don't think I have any further ideas.

Then, on behalf of the University, let me thank you for sharing your recollections about a very important period of time in the University's development.

LUNCHEON CEREMONY DEDICATION OF LYSLE LEACH HALL

University of California, Davis
Friday, October 22, 1982

Opening Remarks
JAMES H. MEYER
Chancellor

Lysle and Jane Leach are here today. It is a pleasure to have you be here for the festivities that will occur in the next day or two revolving around Alumni Day.

My first acquaintance with Lysle Leach was through a series of chairmanships. He was chairman of the Student Conduct Committee, which I joined as an assistant professor, and soon found that there were quite a few things to do, as the students of those days really weren't any better angels than they are today. They did have a few problems. I also found that Lysle was a great friend of students, until one time when the men students formed a group and decided to invade East Hall, which I understood contained women students; and Lysle, of course, being a good dean of students, went over and quieted things down; and as he was leaving, walking down the sidewalk, some young man dashed up behind him with a three gallon bucket of water and poured it over him, and dashed up behind him with a three gallon bucket of water and poured it over him, and dashed on down the street. Lysle grabbed part of his skin shirt off his back. He came next morning to an early start of our Student Conduct Committee meeting, waving this skin shirt, and was very unhappy. It was unfortunate he didn't know whose skin shirt it was, and we never did find out. It was also fortunate that we did not have firing pins in the rifles, or we would have had a firing squad.

My next close acquaintance was when he was chairman of the plant pathology department, and I was dean of the College of Agriculture. He explained a great deal to me about plant pathology. Occasionally, he would convince me that it was the best department in Davis, and sometimes in the United States, and then the next week or two he would have to start over again! He was very patient!! He taught me a great many terms, and a great many diseases and plants. I was able to speak on most any disease of plants for one minute, and that was all; and I can say things like verticillium wilt. Lysle, I don't know what ever happened to verticillium wilt. I found out they haven't cured it yet, or we would not have plant pathologists; but Lysle was an excellent department chairman. The good thing about the college and the good thing about Davis is that we always had good department chairmen, and it was the support and efforts of people like Lysle that built that college and this campus.

Our next major acquaintance was when he was this time chairperson of the Athletic Hall of Fame Selection Committee. He was important to its success [because of] his memory and the fact that he went to most football games, most athletic events over the years. As a matter of fact, he tells me that he attended Davis's football game at Corvallis in 1929. He was a good judge, a good observer, and was able to handle the complaints of the people whom he did not select for the Hall of Fame. He was like that. As you can tell, much of the success of the Hall of Fame was due to Lysle's efforts. He is one of my favorite people, and one of the campus's favorite people; and we are very pleased to recognize him today.

We have a number of other people who want to say a few words. The first one would be [Vice Chancellor of Student Affairs] Tom Dutton.

TOM DUTTON
Vice Chancellor, Student Affairs

Thank you, Jim. It is my great pleasure to participate in this event honoring Lysle Leach. As a battle scarred veteran of the student wars, it is very encouraging for me to know that a fellow administrator can be rewarded in spite of his sins, and can also live to retire and survive, which is one of my goals. Lysle, I questioned your sanity in becoming the dean of students; but I'm glad that you did. When Lysle was dean of students in the 1950s, he dealt with student problems very differently than we do today. He was rather bold and aggressive in confronting students who misbehaved. For example, he intervened directly in water fights; and it is even rumored that once he turned the fire hose on the students. Is that true?

LYSLE LEACH

No.

TOM DUTTON

Rumors, huh? It still makes a good story. Today, we probably exercise a bit more restraint knowing that our students could probably sue us. It probably would be one of our law school graduates that would do it.

Lysle also helped mold the framework for athletics as faculty representative to the Far Western Conference for more than ten years. It is a program we are very proud of, as our athletes have maintained high academic standards, and also have been very successful in athletic competition. Our [football] team is now ranked number four in the entire country, of course in Division Two; and this success has been achieved without athletic scholarships and without sacrificing academic standards.

It is fitting and logical to name our Graduate Residence Hall after Lysle, who made an important contribution to the development of our Residence Hall Program. He established the foundations that have helped make the program one of the best in the entire country, and a program that contributes significantly to the educational objectives of the campus. Lysle Leach Hall, the only facility for single graduate students, houses 178 students, 121 men and 57 women. I would like to make it clear that they all live in separate single rooms. We do not tolerate such foolishness. We are trying to get our graduate enrollment down rather than having it increase so that we think it is important to have this type of facility for our graduate students because it provides an environment conducive to study that is close to our classrooms and the various library facilities. The students also like the facility very, very much. It is a very, very fine facility; and many do not want to leave. We even have one student who is sixty-five years of age. I don't want to mislead you. He really has not been living there all this length of time, and he is not a slow learner. He is simply an academic re-entry student, and he does provide a certain amount of stimulus for the younger people. The students come from all over the United States and the world, making a very cosmopolitan community; and I should add that this dedication has made them all very happy. Their identity crisis has been solved; and after twelve years, Building Number Four [Quatro] has a permanent name.

In conclusion, it is clear that Lysle has contributed immeasurably to the quality of life on the Davis campus in many different roles, and that his effective work has laid the foundation for a very fine quality student life program and environment that exists on this campus today. As the person who must administer the Student Affairs Program, Lysle, we truly thank you for your many achievements and accomplishments. Thank you. (Applause)

CHANCELLOR MEYER

Thank you, Tom. I would like to introduce now, Bill Weir, who is a professor of animal science, professor of nutrition, past dean of students, chairman of the Department of Nutrition, and chair of the Building and Street Maintenance Committee, where he turned a quiescent committee into an activated committee and worked hard because of his past friendship and interest and admiration for Lysle. We'll ask Bill Weir to say a few words now.

BILL WEIR

Thank you, Chancellor Meyer. Lysle and Jane, it is a real privilege to participate today and comment on one chapter of Lysle's many services to UCD. As many of you will recall, 1952 was a year of transition in Davis. The campus had been closed during the war, then we have the postwar plunge, and he was still part of the college. C. B. Hutchison was still running the show along with the help of Ira Smith and a few others, but the residence halls were nearing completion, the students were anxious to move into them. There was a North Wing Club, and a South Wing Club. That was the predecessor of Hughes and Beckett, and so the decision was made that there needed to be a dean of students. It was also decided that it should be a member of the faculty. Looking around, Dean Hutchison went and talked to Lysle because he ideally fitted what they were looking for. He was a respected member of the faculty, had knowledge of Hickey and the athletic program, he was past chair of the Scholarship and Prizes Committee, and so he was appointed [in 1952]. That happened in the same year that Stan Freeborn came, and he had his problems, and he worked with Susan Regan, who at that time was advisor to women but was shortly made dean of women and associate dean of students. At the same time, Burt King came, who many of you will remember was executive director of the ASCA, not the ASUCD. It was the Associated Students Cal Aggies in those days, and he worked very closely with Lysle on many projects including the Leadership Conference, for instance, which is comparable to what the chancellor now has with his conference in the fall. Lysle was a real advocate of students. He worked actively with the Executive Committee. Ed Spafford was president of the ASCA in that era. He worked as Welfare Council advisor. We gave a lot of responsibility to the Welfare Council in those days, and they handled a lot of very sticky cases, as many of you know. He also worked with the Interfraternity Council.

Lysle and Susan were active on the Living Accommodations Committee and this presented some challenges. They had an annual guessing game as to how many men and women would come; and this was important because in those old fashioned days they thought the women should be in one hall and men should be in another hall, so they had to keep playing checkers with who was in which hall. Those of you that were around in those days remember the new halls were given to men; and the women were in East Hall, West Hall, A, B, Cs [Ash, Birch and Cedar Halls] and so forth. But the women kept coming, so they made a real radical decision. They put men on one side and women on the other side, and all kinds of dire predictions were made when they moved women into Hughes Hall; but it all came off all right. I think it's still that way. I don't know how Charles has got it organized these days. I hear lots of rumors, but I don't ask any questions. But Lysle played a real part in developing the whole residence hall system. Struve was built even though one of the floors fell down. Those of you that were around in those days will remember it was one of these tilt floors that you push up. They got it up and it sagged, and so they had to start all over again. It eventually got built. Titus was planned and built, and then the Secundos came along.

Now I have a punch line that I was going to tell on Lysle because when I took over for him at the point he went sabbatical, in the briefing session, he gave all these sage words of advice on what I should do and what I shouldn't do, and how important what I said might be and how I might influence some kid's life, and so on. Then he showed me his trophy. He pulled out the drawer, and it's the same trophy the chancellor has already told you about.

Seriously, Lysle played a real part in the very happy epoch in student affairs, and I can understand his desire to go back to the plant pathology department and I would like to say "Thank you." During the years that I was dean, he was a lot of help. I could always turn to Lysle and say, "What do I do now," so it [this honor] is well deserved, and we appreciate him.

CHANCELLOR MEYER

The theoretical framework for student affairs today is that it is better not to know, but that's going to change.

I do want to introduce now, Jack Oswald, who is a plant pathologist and erstwhile assistant football coach, chairman of Department of Plant Pathology, assistant to two chancellors and vice chancellor and vice president, president of the University of Kentucky, executive vice president of the University of California, and now president of Penn State University. He is out of here, so we take advantage of him because he was an early member of the Department of Plant Pathology—Jack.

JACK OSWALD

May I say to Jane and to Lysle, and the entire Leach family, how thrilled Rose and I are to be here on this occasion because there couldn't be an occasion more right for the University than to have Lysle Leach honored in such a way. And I thank you, Jim Meyer, more than I can say for permitting me to participate in this function honoring one of the special people in my life; and at the same time, joining a truly fine hour for the Davis campus. One key reason, in my own view, that the University of California at Davis has evolved so admirably into both a sizable university with the highest of quality is the care, concern, and warmth for its people; and that has developed because of the large number of people like Lysle Leach serving it.

I came to Davis first in 1938 as a graduate student. I asked the chancellor last night a little about the history of the University; and he said that I came two years after the first graduate student, which he informed me, of course, was in animal sciences in 1936. But there were a group of us that came about that same period, and we clearly thought of ourselves in every way as students of the Davis campus, even though the official name at that time was the University Farm. I certainly was aware also that my diploma, as hard as I looked for the word "Davis" on it, says Berkeley. I guess that is all part of growth, but, nevertheless, I am a Davis graduate I early became part of Lysle's research team, and soon almost a part of his family. I early experienced the special concern about others that characterized Davis and this emerging university, and that Lysle was one of the leaders in providing that kind of attitude. Not only did I learn professionally from Lysle, but I learned early of his special concern for students.

After World War II, Lysle, you and Jane took Rose into the family just as you had taken me. Rose used to laugh how Lysle and I acted somewhat like glue in those days. She would observe that each night it took us about ten minutes to walk home, and usually another fifty minutes to close off our conversation. In those talks I learned of Lysle's broad interests as we tried to solve, of course, all of the problems of Davis, and also beyond. I learned of his deep interest in sports as he second-guessed, properly so, many of the football coaching decisions that I had tried to make the previous weekend; and, I might add, that it was my coaching experience here at Davis that came very close to costing me my tenure. Usually a coach ceases to coach, unless he is ready to retire because of age or after a year in which success has not been overwhelming. But, in my instance, I ceased my coaching activities right at the peak of one of the better teams—not the equal of today's fine team in Davis; but in those days I guess if there was a division three or four, that's where we would have been. We tried just as hard as Penn State or the current UC Davis team. But my giving up coaching had a lot to do with three or four professors who were watching my progress, or lack thereof, in plant pathology, and wondered whether or not I had planned to coach as a career or to become an academic professor; and I must say I made up my mind rather quickly. Nevertheless, Lysle was a tremendous help in his critique of my coaching, or lack thereof.

Lysle also was the one who first sparked my own interest, and I am sure that of many others, in international education. The Fulbright Program, which has been such a boon to so many thousands of young scientists from around this country to various spots around the world, started in 1952. It was Lysle who convinced Rose and me that this was a program that we should take advantage of. It fit exactly the work in which I was involved, and in 1953 we found ourselves in Holland, I believe in the second year, perhaps maybe one of the first of the Fulbright scholars from the Davis campus. Whether or not that was the case, it sparked my interest to really a high point some twenty years later, when in Moscow I chaired a session at the International Association of Universities of some 700 rectors from both sides of the Iron Curtain. It was one of the thrills of my lifetime, and one of the most fascinating experiences I have ever had, and indeed I look back to many early discussions with Lysle about the value of international education. Lysle himself in this area has made tremendous contributions in many countries—Chile and Ireland, for example, and many others. Although he has spent his total professional life here at Davis, far-flung parts of the world have felt touched by the abilities of this man; and so it is with a real, real thrill that I add my congratulations to Lysle Leach, a truly all-around human being, on the dedication of this residence hall in his name. Congratulations, Lysle. (Applause.)

CHANCELLOR JAMES MEYER

Thank you, Jack. It is traditional for the campus to display a photograph in facilities it names for individuals. We do, however, have the following citation placed in Lysle Leach Hall; and I would like to read it:

"Lysle D. Leach served on the faculty of the University of California as a teacher, researcher, student advisor, and administrator for forty years. He was Chairman of the Department of Plant Pathology from 1960 to 1968, the first Dean of Students on the Davis campus serving in that post from 1952 to 1958. In 1955, he was named Faculty Research Lecturer by the Academic Senate, that body's highest honor. For many years, he served as the Campus Faculty Representative in the Far Western Intercollegiate Athletic Conference. Professor Leach's research on fungal diseases in sugar beets and cotton gained him international renown. He retired in 1968. In his honor, this complex is named Lysle Leach Hall."

I will now ask Lysle to join me in unveiling the portrait which will be displayed beginning this afternoon in Lysle Leach Hall. (Portrait unveiled; Applause.) I now declare that Lysle Leach Hall is officially dedicated; and Lysle, your many friends who are present here today would enjoy hearing from you. The microphone is yours.

LYSLE LEACH

Thank you. It seems to me that almost everything has been said, probably more than should be said. I was rather anxious to come here and find out what these people thought I had done to deserve this honor. Now you've heard them, and you can be the judges. I do want to thank Chancellor Meyer for making the recommendation to the Regents, and I thank the Regents for approving it. I also want to thank the others who participated in this decision even though they are, in general, unidentified. I particularly want to thank the people who have been primarily responsible for planning and carrying out this luncheon and the dedication. I know that Mrs. Kay Gary handled all the reservations. Bob Bynum handled publicity, John Hardie worked with overall planning and I am sure there were others whom I do not know that took part. My thanks to all of them. Then I want to thank all of you who are friends of many years who took the time to attend this dedication.

Some years ago, Jane and I were at the University of Bologna [in Italy] and there we learned how they honored their professors. It seems that upon their death they were buried in a vault which was raised on stone pillars about eight feet above the ground, and these vaults were scattered between the buildings on the campus. I assume that this is so these selected professors could look down on the future generations of students. For my part, I think it is much better to have your name given to a students' residence hall that will at the present and in the future house graduate students; and we hope that they will find the conditions for living and studying very satisfactory. They appear so to us, and we hope that these very good students will find time in their busy lives for some social activities and also for participating in activities on the Davis campus.

This gathering is very special in another way, that is, for the number of emeritus professors who are present where campus buildings have been dedicated in their names; and I will mention these: Knowles Ryerson, Emil Mrak, Roy Bainer, Vern Hickey, Ed Roessler. In addition, there are wives of several former professors whose names appear on campus buildings—Dr. Ruth Storer, Mrs. Dorothy Briggs, Mrs. Maxine Young, Mrs. Cynthia Cole; and if I have overlooked anyone, I hope you will either correct me or forgive me.

It seems to me that my university life has been divided into three rather closely related parts which I would term teaching, research and student activities. I taught in a junior high school for one semester, in a senior high school for three years, and in the University at the lower division, upper division and graduate levels. At each of these levels, the preparation was more rigorous than the one just below it; but the rewards were also much greater. At the upper division and at the graduate student level, I had the satisfaction of knowing that any help I could offer would be added to, improved upon, and communicated to others in their teaching and research publications. In the area of research, this offered the opportunity to work with some very intelligent graduate students; and it offered also the satisfaction of occasionally solving problems that would benefit someone and in the process occasionally developing new information or new ideas. The third area was my contact with the student activities or student groups, and you have heard considerably about this already, this mainly from scholarship committee to intercollegiate athletics; and I would like to say on the latter deal, that I have been very proud that the Davis campus has been able to attract such high quality student athletes, and compete so successfully in both men's and women's sports without the offering of grants-in-aid, sometimes called athletic scholarships.

In 1951 Claude B. Hutchison, who was vice president of agricultural sciences, asked me if I would accept a half-time appointment as dean of students. He said, "This is not a life sentence, but I would not want you to accept for only one or two years." The dean of students office was shared by Mrs. Susan Regan, mother of Mary Regan Meyer; and Mrs. Regan, as you have heard, was the first dean of women. Any success we had in that office was due in a large part to the very effective working relations that Susan Regan established with the women students, and especially with the student leaders. We were both interested in student housing and served on the committee that planned the last two of the Primero Residence Halls and the four units of Segundo, that is, the four high rise units and the central commons. I was very pleased when Susan Regan and her husband, Bill, who was a professor of animal husbandry, were honored by the naming of Regan Hall. In 1958, Chancellor Freeborn was planning to retire; and it seemed an opportune time to ask to be relieved of being dean of students so I could return to full-time plant pathology for the last ten years before my retirement. As the Cal Aggie put it: "Dean Leach Steps Down to Research." This decision was made easy by the indications that Bill Weir would accept the position, so I knew that I would be leaving the office in very capable hands.

You've heard today about the water fight. It seems to be one thing that most people remember about my tenure as dean of students; but they didn't tell quite all the story. The next day I received in the mail a condolence card which said, among other things, "Into every life a little rain must fall" and included a prepaid bill at the cleaners for a suit.

I want to share this special occasion and this honor with my wife Jane, for without her companionship and support, all of this would not have been possible. But, as I heard Chancellor Meyer say, she was principally responsible, I am really going to have to renegotiate. I also want to share this occasion with our family who are seated at this table, and particularly for those who are able to attend, and for the others who are unable to be here today because of distance or other commitments. Again, I want to thank you for this very pleasant occasion; and I want to thank all of you for coming. Thank you.

CHANCELLOR MEYER

Thank you, Lysle. Now this will conclude our program. Lysle and Jane will be departing shortly for Lysle Leach Hall and will be pleased to greet you there. Thank you very much for coming. This concludes the program.

Lysle Leach Hall, a six-building complex, was completed in 1970 to house 178 graduate students. Located on Bioletti Way south of the Tercero residential hall complex on the west side of the central campus, the Hall was designed by Joseph Esherick and Associates, Inc., of San Francisco.

LYSLE LEACH HALL DEDICATION

October 22, 1982

Lysle D. Leach

Professor of Plant Pathology, Emeritus

Lysle D. Leach served on the faculty of the University of California, Davis as a teacher, researcher, student adviser and administrator for 40 years. He was chair of the department of plant pathology from 1960 to 1968 and first dean of students on the Davis campus, serving in that post from 1952 to 1958. In 1955 he was named Faculty Research Lecturer by the Academic Senate, that body's highest honor. For many years he served as the campus faculty representative to the Far Western Intercollegiate Athletic Conference. Professor Leach's research on fungal diseases of sugar beets and cotton gained him international renown. He retired in 1968.

APPENDIX

THE BEGINNING OF MODERN PLANT PATHOLOGY IN CALIFORNIA

By Ralph E. Smith

[Excerpts from Professor Smith's unpublished manuscript, prepared in 1953 in commemoration of the fiftieth anniversary of his arrival in California and of the Department of Plant Pathology, which he founded at the University of California.]

With the exception of Pierce's notable work, and the limited activity of Woodworth and Bioletti in this field, there was no professional plant pathology or research on plant diseases in California prior to the year 1903. At that time I was Assistant Professor of Botany at the Massachusetts Agricultural College, having graduated there in 1894. My training in plant pathology consisted of studies with Professor George E. Stone, two summer schools at Harvard University with Professor Farlow and Mr. A. B. Seymour, a year in Munich, Germany, with Professors Robert Hartig and Karl von Tubeuf, and several years of investigational work at the Massachusetts Agricultural Experiment Station.

My coming to California was rather unexpected or accidental. I read in a California Experiment Station report by Professor Hilgard, of the recent appearance in California of the rust disease of asparagus and that this offered a serious threat to the asparagus canning industry of the State and something should be done about it. We had had asparagus rust in Massachusetts for several years and I had assisted Dr. Stone in preparing a bulletin about it (Massachusetts Bulletin 165). Having the usual yen to migrate to the "Golden State" and feeling myself as competent as anyone for the task at hand, I wrote to Dr. Hilgard offering my services as the savior of the asparagus, and was duly invited to come, with a prospective appointment as Assistant Professor of Plant Pathology. I also had correspondence with Dr. Setchell and there was some uncertainty whether plant pathology belonged in Agriculture or Botany.

On the morning of April 1, 1903, I strolled up to Budd Hall, through the, to me, tropical vegetation of palm trees and blue gums, and therewith (I say it with modesty but as an undeniable fact), for better or for worse, Plant Pathology on a professional, permanent basis came to California. I was not so sure of this a little later the same day. It seemed that although an appointment as Assistant Professor of Plant Pathology had been offered to me the administration had not made this official, owing to the fact that funds for the remainder of the current fiscal year had been promised by the asparagus people but no money had been paid in. And Boston three thousand miles away! This matter was soon adjusted however and my appointment confirmed.

So far as I know, I was the first to bear my new title in any American college, and mine was the first separate department of Plant Pathology in existence. L. R. Jones was still in Vermont, where W. A. Orton, Lutman and Giddings were among his early students. Atkinson and Duggar were at Cornell, Bessey at Nebraska, Bolley at North Dakota, Freeman at Minnesota, Pammel in Iowa, Halstead in New Jersey, Sturgis or Clinton in Connecticut, and all bore the title of Botanist. Galloway, Pierce, Wood, Waite, E. F. Smith, Webber, and Swingle were in the Federal service and they were titled Plant Pathologists.

During my first day at the University I met Hilgard, Wickson, Jaffa, Loughridge, Woodworth, Ward, Colby, Major, and others, also President Benjamin Ide Wheeler, whom I got to know well in later years. I served with four presidents and five deans of Agriculture of the University of California and to me Wheeler was, and still is, the big man of that institution. He always had (and knew it) perfect command of every situation. Apparently rather cold, arbitrary and domineering, he was at heart human, friendly and eager to inform himself of the true facts on any subject. I greatly admired and liked him, although sometimes I was a little afraid of him.

The Agriculture and Botany groups, of which I soon became very much a part, were most friendly and cooperative. With the rest of the University faculty I never felt entirely at home, perhaps on account of having been exposed too long to the provincial atmosphere of a small New England college . . . .

Wickson was always my favorite among my faculty associates, with his great gift for informative and entertaining writing and agreeable companionship. On the day of the San Francisco earthquake we had an appointment in Sacramento with Governor Pardee regarding the choice of locations for the University Farm, the Citrus Experiment Station, and the Southern California Pathological Laboratory. It was many months before that appointment was kept.

Two incidents will illustrate Wickson's subtle wit. When the College of Agriculture was reorganized in 1912 with the coming of Dean Hunt and several new faculty members from the East, there was a little feeling that the prestige of the new members was being promoted somewhat by the disparagement of the older ones. At a faculty dinner at which Professor Wickson was to speak, he rose from the table with a Bible in his hand and announced that when asked to say anything in public he always liked to choose a text for his remarks. For the present occasion, he said, he had taken his text from the Bible and it was composed of two parts: "And the Lord brought an east wind upon the land." Exodus 10. 13. "Should a wise man utter vain knowledge and fill his belly with the east wind?" Job 15. 2.

The other illustration was in an editorial in the Pacific Rural Press. Referring to an unpopular State official, Wickson said, "It is going almost too far to charge that he is incompetent or dishonest." Nobody had publicly so charged, but everybody wanted someone else to do it . . .

Since the University was counting on the asparagus money for the support of my work, a delicate suggestion was made that I call on some of the leaders of the industry and diplomatically dun them for the money. My second day in California was spent, therefore, in my first visit to San Francisco and an interview with Mr. Robert L. Bentley, who was at that time head of the California Fruit Canners' Association, now the California Packing Corporation. By now I was beginning to feel a little important, since in San Francisco the rating of plant pathology as a matter of practical importance seemed to be in a considerably higher bracket than in Massachusetts. I think that one of the proudest moments of my life was when, after taking me to lunch, Mr. Bentley escorted me in person to the office of Mr. William Sproule, head of the Southern Pacific Railroad, and suggested that an annual pass over the road would greatly facilitate the work that I was to do, work that would undoubtedly increase the freight business of the Southern Pacific. Anyone who knew those men, "R. I." Bentley and William Sproule, will appreciate my feelings on that occasion . . . .

After visiting and establishing connections in Milpitas, my next objective was the delta country, which was the principal asparagus district in California, and the one which I wanted most to see. . . . To establish my work in the delta, it was suggested that I contact Dr. Robert Hickmott, a large grower and canner of asparagus and competitor of the Growers' Association.

At his office in San Francisco, I found him a strong-minded, red-haired, somewhat irascible, belligerent, energetic individual who, like the Milpitas Chinese, was much disturbed about asparagus rust which was just making its first appearance in the delta. We made an early appointment to go, by stern-wheel river steamer, to Mr. Hickmott's property on Bouldin Island, a few hours' trip from San Francisco. It was a beautiful property, with several hundred acres of asparagus in peak condition and two canneries for packing the product. I had a home there whenever and as long as I wanted and perfect cooperation. Mr. Hickmott also paid his share of the subscription to the university.

If I did not yet realize the responsibility of my position, I had one day a sharp reminder of it. Riding on the river about the island in a motor boat with my host, he suddenly stopped the boat and pointed dramatically to the island. "Professor," he said, "there is everything I have in the world and it is up to you to save it." Again my horizon widened . . . .

And now began my real work, for which I had come, with a good deal of ballyhoo, from Massachusetts to California. At the University I was given a comfortable room in the basement of the old botany building, and a student microscope and simple equipment was loaned by the Botany Department, which then consisted of Setchell, Jepson, Osterhaut, N.L. Gardner, H.M. Hall, and Miss Katherine Jones. The university bought me a cheap camera, with which I took all the pictures in Bulletin 163 and 165 (1905), including developing the plates and making the prints. I also made the drawings.

Like many another apparently hopeless situation, the asparagus rust problem was solved with comparative ease. First it was found that although spraying the tops by conventional methods with liquid fungicides was too cumbersome and slow for practical use, and not very effective in rust control at that, the fields could be dusted with finely pulverized, dry sulfur rapidly and cheaply and with good results in rust control. Mr. Boots, my clever cooperator, hit on the idea of using a modified rotary seed sower to distribute the sulfur, and rust control in California soon became an inexpensive, routine, mechanical operation . . .

No living plant pathologist could have remained long in a rich, unworked field like California a half century ago without becoming interested in more than one disease. If there existed such a one the public would, very soon after his arrival, have called to his attention a multitude of problems and questions which urgently needed attention. And in my case they were such a nice, friendly, agreeable public! A call to examine a sick tree didn't mean an auto trip out and back in a few hours, or several calls in one day. One went by trains, was met at the local station by a horse-drawn rig, put in the best room in the house or, on the big ranches, into a clean, comfortable room in the bunk house, ate the best meals served on the place, often with a choice of one's favorite dishes, and stayed a week.

As to taking up new problems, it seemed in my own case highly desirable to strengthen my position in the State and University as well as to serve the public by further demonstrations of the practical possibilities of plant pathology. Really I had little choice in the matter, so strong was the demand for information. Rather naturally, my choice fell on the disease called pear scab, since it was of great economic importance, some of the asparagus growers had pear orchards and were much interested in the problem, and the disease was not being satisfactorily handled here . . . .

Here, as always, the fullest cooperation was received and we were able the first season to show that control of pear scab on the fruit depends not so much on the spray formula used, so long as it is a fungicide, as on the time of spraying in relation to the development of the blossom buds. The first application should be made when the buds start to swell and show pink color, followed by one or two more sprayings during the blossoming period. This simple treatment offered an almost certain guarantee against scab, and, as regards expense, could be combined in part with the spraying necessary to prevent damage by worms. A bulletin was published (California Experiment Station Bulletin 163, 1905), describing the procedure and containing photographs of the critical blossom bud stages, which proved to be a very useful and popular feature of the work.

During the summer of 1904, while I was still the only active plant pathologist in California (or on the Pacific Coast), something of the nature of a disaster occurred to the pear orchards of Northern California, something comparable in importance to the Phylloxera and California vine disease of many years earlier. This was a virulent and extensive outbreak of fire blight, that scourge of the apple and pear industry in many parts of America. In California, this disease had been reported intermittently, but in most if not all cases erroneously, for many years . . . . In the great Bartlett pear orchards of the Sacramento Valley, comprising thousands of acres of perfect trees, blight had never been recognized.

But in 1904 something new happened. Reports and specimens began to reach me at the University, originating all the way from Concord in Contra Costa County on the west to Grass Valley on the east, and from Modesto to Redding. The Sacramento Valley orchards were badly affected. A virtual panic began to develop.

Pear blight is a bacterial infection which, in active cases, quickly kills affected branches or trees and spreads rapidly from tree to tree, the germs being carried by insects which visit the blossoms or tender green shoots. The bacterial cause of pear blight, and the first instance of the kind in any plant disease, was proven by Professor T. J. Burrill of Illinois about 1880. His student, Merton B. Waite, demonstrated what is still the only effective method of control, one that is exacting but certainly proven in California to be successful when properly applied. The natural procedure for an inexperienced person trying to arrest pear blight when it is developing and spreading rapidly is to walk through the orchard looking for blighted twigs, cut these off and burn them, and, if he is extra careful, treat the cuts and tools with a disinfectant . . . . It will be seen that the success of this work depended upon the degree of care and skill exercised in the inspection and surgical work.

The legislature of 1905 made an appropriation of $10,000 to the Division of Plant Pathology of the University for pear blight work. Illustrative of the simpler practices of a half century ago compared with today, may describe the manner of getting this and similar appropriations. Mr. Alden Anderson, an important banker, fruit grower and politician of that day (Jack Z. Anderson, pear grower and U.S. Congressman of Hollister is his nephew) was Lieutenant Governor and President of the State Senate. I wrote a bill, stating what we wanted, sent it to Mr. Anderson and he put it through. The University authorities never heard of it until they got the money.

Being in Sacramento one day while the legislature was in session, I called on Mr. Anderson at the State Capitol. He was presiding in the Senate. When I sent up my name he invited me to the rostrum to talk with him during some rather long-winded speeches. When he was called out for a few moments, a member rose and, addressing the chair, said "Mr. President." As I was the only one on the rostrum, I was quite flabbergasted for the moment, but having seen Mr. Anderson's technique on similar occasions, I made an affirmative gesture and the debate proceeded. Little did I realize that when I came to California as Assistant Professor of Plant Pathology that the duties would include presiding over the State Senate.

Realizing the seriousness of the pear blight situation by the fall of 1904, as did also State and County Horticultural Commissioners, I sent an S O S to my old friend in Washington, Mr. Waite. He was one of the first trained plant pathologists in the United States, a good scientist with the prestige of having been a student of Professor Burrill, a commercial fruit grower himself, and a man with a wonderful gift of interesting and instructing the public in any message which he wanted to convey. Mr. Elwood Cooper, our State Horticultural Commissioner, also solicited the help of Mr. Waite.

Early in 1905 Waite came to California, met with various interested parties, and eventually went into the field with the two assistants employed by the Division to instruct them in methods of pear blight control. These men were W. H. Volck, afterward one of America's leading spray chemical experts, and Burle Jones, who later became a county farm advisor. This group was gradually enlarged. Waite brought out from Washington a number of top men like C. L. Shear, W. M. Scott, W. A. Orton, Walter Ballard, P. J. O'Gara, and others, and these, with University men, were scattered over the pear districts to help and instruct growers and their employees, County Horticultural Commissioner, and others.

For the University's share in this combined effort I wanted to get some responsible men with, if possible, training in plant pathology, with the idea that some of them might develop into permanent members of the Experiment Station staff. That this plan worked out even better than expected will appear later. Since there were no local graduates in plant pathology in California, the best possibilities of which I could think were some of my former students in Massachusetts. The class of 1905, whom I knew well, were just graduating and no doubt looking for jobs. By corresponding with Dr. Stone I secured four men and subsequently a fifth from this source. For later reference I will state that these men were Richard L. Adams, Thomas Francis Hunt, Norman B. Ingham, Fred L. Yeaw, and Stanley S. Rogers. I also got one California man, Ernest B. Babcock, who was my first student in plant pathology, and one from the Experiment Station at Geneva, New York, Henry J. Ramsey. For the pear blight work we set up a training camp at Vacaville, where pear blight was abundant and virulent, living facilities good, and the grower cooperative. The boys liked it here and combined hard work with recreation. They had a baseball team and challenged the local players. Hunt and Ingham formed one of the best amateur batteries in California.

It was demonstrated that the general application of pear blight control methods required, with all growers *education,* with some *persuasion,* with a few *force* (Bulletin 184, p. 231). That we obtained good results in this difficult task and others which followed was largely due to the efficiency, energy, tact and good judgment displayed by these young men. Although none of them had had advanced, technical training in plant pathology, they constituted one of the most powerful influences in the establishment of practical, successful control of devastating plant diseases for which California has become famous. They were like sons or younger brothers to me and the untimely deaths of Hunt, Ingham, and Ramsey was one of my deepest regrets. I employed for laboratory research and office work, and later for teaching, Miss Elizabeth H. Smith [Professor Smith's sister], a graduate of Smith College, with a Master's degree from Massachusetts Agricultural College. With most of us in the field a large part of the time, she was the mainspring of our group for many years until her accidental death in 1929.

Pear blight and the work of State and Federal men, stationed in the various pear growing centers, went on throughout 1905 and 1906 and into 1907. Mr. Waite came to California several times and he and I visited the men in the different districts. He traveled hard, trying to cover the ground as thoroughly as possible, with long hours, irregular meals, and much physical exertion, once with almost tragic results [an appendicitis attack] . . . .

In 1908 the blight began to taper off in virulence and disappeared almost as suddenly as it had come, leaving a vast amount of wreckage of fine pear orchards. It has recurred occasionally at time and places, but the next major epidemic was not until 1930. During the campaign of 1905-1907, our boys had inspected about three quarters of a million trees and done surgical work on a great many of them. Meantime the education received by the growers in the first great attack enabled them to carry on successfully and now pear blight control is almost a routine operation. We felt that our young experts of 1905, under Merton Waite's direction, had won a major battle in plant protection.

We speak of "breaks"—good ones and bad ones. Certainly the year 1905, in relation to plant pathology in California, had good breaks galore. Speaking for the grower, he found at the University service on plant diseases which he had never had before and good control methods for asparagus rust and pear scab for growers of those crops. For the pathologist, good results in two important projects, popular support and cooperation in his work, financial and otherwise, a good background of important new problems, and a promising, loyal crew of trainees to help him.

Before the pear blight excitement got underway, I was invited to attend a meeting of walnut growers at Los Angeles to discuss walnut blight. This was not, of course, because I had any knowledge of walnuts or blight, but simply because the news was getting around that work on plant diseases had been started at Berkeley . . . This meeting was very valuable to me in that it made me acquainted with some of the old timers of the walnut business; Tom Cooch and Bert Montgomery of Rivera, Ralph McNess of Whittier, B. F. Porter of Fullerton, J. B. Neff of Anaheim, C. C. Teague and Carlyle Thrope of Santa Paula, George Williams of Santa Barbara, and others. This was several years before the foundation of the California Walnut Growers Association in 1912. Before the meeting adjourned they voted a contribution of $3,000 to the University of California for work on walnut blight, as hopeless a job as could be selected and one which Pierce, who discovered the bacterial cause of the disease, had already given up. This donation was later withdrawn in favor of a $5,000 appropriation for the same purpose made by the beneficient legislature of 1905 . . . .

During World War I, I moved my family to southern California and spent three years there working on walnut problems with the Association.

Although it has been talked about and looked for unsuccessfully during the past 50 years, the finding of a good walnut resistant to blight still seems not impossible and it would certainly be the most desirable solution of the problem . . . . There are still many seedling trees in the State, so a chance exists that we already have the ideal walnut, waiting to be discovered, propagated and tested. If so, I earnestly hope that someone will find it and name the new blight-proof variety "Smith" as a memorial to one who admired the walnut above all other products. But it *must* be *good.* There are still, of course, the possibilities of "planned parenthood," definite crossing and breeding for the ideal nut, but I doubt if I shall ever enjoy the fruit of that dream unless I am granted another half century of activity . . . .

At the close of the walnut growers' meeting in Los Angeles, one of the members, Mr. C. C. Teague of Santa Paula, asked me to remain for a further conference. He told me what everybody else there knew, that he was a grower of lemons as well as walnuts, and had a pathological problem in that crop also, a peculiar rotting of the fruit in storage which was raising havoc with the industry. This trouble had appeared in the last few years just when the business, after many vicissitudes, was getting into a prosperous condition. Mr. Teague invited me to go to Santa Paula with him, which I did, and there I saw the Limonaira Lemon Company which, if not the finest agricultural property in the United States, was certainly at the top. And there, as on many later occasions, nothing was too good for me and I was a guest of honor.

The work which we planned was given support by the other lemon houses in southern California through an organization called the Lemon Mens' Club, both by furnishing facilities for experimental work and contributions to a fund of $1000 which they pledged to the

University. We wrote our own ticket in those days, thanks to the abundance of good problems and the splendid cooperation which could always be counted on from the industry concerned.

Lemons are usually taken from the tree when they reach a certain size, being still green and hard. They are washed in large tanks of water, then stored for curing in tanks or chambers where the humidity can be controlled. During curing, the juice increases and the rind decreases. It is in the curing process that most of the "brown rot," as it is called, develops. Fruit which look perfectly sound when put into storage may develop as much as 25 or 30% rot, and any lemon which touches an affected one is almost sure to go down, so that the loss multiplies rapidly. This trouble was first noticed about 1901, and had increased steadily since that time.

Investigations by Miss Smith in Berkeley, C. O. Smith and J. J. Ramsey at Limonaire, and E. B. Babcock in Whittier, soon showed the following facts. Brown rot is caused by a hitherto undescribed fungus of the water mold type, now named *Phytophthora* (Pythiacystis) *citrophthora*. This fungus lives in moist soil. It is carried into the wash water in orchard soil clinging to the lemons and picking boxes. Disinfection of this wash water with small amounts of bluestone (copper sulfate) or other chemicals, or by heat, is easy, cheap, and effective.

These results solved the brown rot problem completely. A bulletin on the subject (California Agricultural Experiment Station Bulletin 190) was published in 1907. This has always seemed to me one of the best problems that plant pathologists have ever encountered, best for the grower because means were given him for controlling completely, at trifling expense and with a minimum of effort, a very destructive disease. The savings to the lemon growers every year has been many, many times the cost of the work. Best for the pathologist because he was able to bring this about quickly, completely, and economically, both as to the cause and control of brown rot.

If the pathological events of the extraordinary year 1905 in California were not sufficient, there are more to follow. Early that year I was introduced by Professor George W. Shaw to one of the most puzzling and destructive plant maladies that has ever beset the American farmer. That was the famous curly top of the sugar beet, which was new to me and to scientific literature, but not to California. Dr. Shaw, grabbing like everybody else for a piece of the new plant pathologist, invited me to go to Salinas and become acquainted with this scourge of the sugar beet. There I met Mr. Ralph Moore, local manager of the Spreckels Sugar Company, and formed a personal acquaintance and friendship with him and his family which was to endure throughout our respective lives. Of the Spreckels Company, founded by Claus Spreckels, Sr., it could be said that, with a single reservation, it was one of the most highly developed and successful agricultural enterprises in California. With thousands of acres of sugar beets on excellent soil, scattered from Watsonville to beyond King City on either side of the Salinas River, an ideal climate, served by a private railroad, and with a mammoth mill in its midst, it would seem to have constituted a perfect set-up for the production of beet sugar. The one drawback was the curly top or, as we called it then, blight disease of the sugar beet plants, the cause and nature of which, not even to mention control, was a complete mystery in 1905. The disease caused a withering and blasting of the young plants in a very characteristic manner, after all the preliminary expense of soil preparation, planting, cultivation, and irrigation had been incurred. It was extremely sporadic, breaking out violently in some seasons and not at all in others. It tended to be less serious toward the coast and more prevalent inland. Thus, at Castroville or Watsonville beets might be practically normal, but badly diseased at King City. Beets planted early in the spring usually fared better than later-planted ones. In the Sacramento and San Joaquin valleys beet culture, otherwise successful, was hopeless on account of blight, and several mills had been built and abandoned there. The disease also prevailed in Utah, Colorado, and in any sem-arid interior region where sugar beet culture might be attempted.

In 1899 Mr. Claus Spreckels had sent affected sugar beets, preserved in alcohol, for examination to scientists in Germany, but with no tangible results. The idea was expressed that the disease might be a bacterial one.

After our consultation with Mr. Moore and others, the Spreckels Company offered to add another major project to our responsibilities by furnishing money to start an investigation of "beet blight" and we kept men in residence at Spreckels for several years. Most of the pear blight workers took part at times, particularly Ramsey, Hunt, Adams, and Rogers, together with various others at later periods. Eventually the Spreckels Company organized its own experiment station and took over some of these men and others. Also a Federal entomologist, C. F. Stahl, was there at an early date and Eubanks Carsner, a pathologist. Some of our earliest conclusions about curly top were expressed as follows (California Bulletin 184, p. 241, 1907).

"The conclusion reached . . . has been that the trouble is not brought about by any parasite or organism, but is rather due to a derangement in the normal functions of the plant which cannot be fully explained. The disease has a very marked relation to moisture and climate, especially rainfall at certain seasons, but presents many very peculiar features. It is a very definite disease, with characteristic symptoms, and not simply the injurious effects of unfavorable conditions. The disease is one of a number of so-called 'physiological' plant diseases . . . ."

The first break in curly top came in 1905, when Dr. E. D. Ball, entomologist of the Utah Experiment Station, discovered that this disease followed the attack on beets of a minute insect, a leafhopper *(Eutettix tenellus)* and that the vagaries of the disease in relation to weather and other factors were due, not to direct effect of such influences on beet plants, but to effects on the abundance and behavior of the insects. This was soon corroborated at Spreckels, following which the men working there demonstrated that curly top was not a direct injury due to the insect feeding on the beet plants, but that *Eutettix* was the carrier of a virus which was not present in its body until it had fed on a previously affected plant. It was also found that *Eutettix* had many hosts, both cultivated and native, besides the beet, and incited diseases in some of these, like the "Western yellow blight," a very serious disease of tomato. Thus the problem of control seemed to switch from pathology to entomology. Much valuable work upon *Eutettix* was done, especially by Dr. H. H. P. Severin, but no practical way was found to attack the insect. Then followed the plant breeders, both Federal and State, and through their efforts satisfactory, curly top resistant varieties of sugar beets were produced, which completely solved the problems, one of the major accomplishments in the whole field of plant protection.

The extraordinary list of troubles which broke out in 1905 had still another member, the disease called peach blight, or sometimes "winter" blight or "gum" blight. In 1905 and again in 1906 this occurred to an alarming extent in the interior valleys . . . .

The legislature of 1905 took other action which affected the future of plant pathology in California. The University Farm at Davis (Branch College of Agriculture), the Citrus Experiment Station at Riverside, and the southern California Pathological Laboratory at Whittier were established. We had only a small part in the first mentioned. A large committee of State and University officials and others was named to choose a location. They diligently toured the state examining many proposed sites for the Farm, but were utterly unable to agree. Typical was the attitude of Mr. Elwood Cooper, that genial old-schooled gentleman who was the State Commissioner of Horticulture. When preference was suggested for any other site, he would always say, "What's the matter with Santa Barbara?" The choice was finally left to Professor Wickson and time has proved the wisdom of his decision. He immediately named the "Davisville" site for what was then intended to be a "secondary school of practical agriculture," to affect the rarefied, highbrow atmosphere of Berkeley. If my memory is correct regarding the original purchase, the state paid about $97 an acre for a little less than a section of land, but these figures are subject to correction.

As to southern California work, I was named "Superintendent of Southern California Pathological Laboratory and Experiment Stations," in addition to my former title, which put me in charge of the final days of two of the last relics of the University's old system of sub-stations, the Santa Monica forestry station and the Pomona (Chino) sub-station. I well remember an enjoyable day spent inspecting the former with President Wheeler and Professor Wickson, driving out to Santa Monica canyon, where the station was located, in a livery stable conveyance. A short time later the Imperial Valley station was created and added to my list. It might not be too far from the truth to say that the plant pathology underground system of legislation had something to do with this . . . .

The original Citrus Experiment Station at Rubidoux Heights and the Southern California Pathological Laboratory at Whittier were created by the legislature of 1905 in response to popular demands for University service in these fields. The organization and conduct of these useful institutions, which were merged into the greater Citrus Experiment Station in 1912, was entrusted to the Division of Plant Pathology. I made my home in Whittier from 1906 to 1912, commuting to Berkeley. I was married and my son was born there.

That the achievements of my first group of associates were by no means accidental is attested by their further record. When Dean Hunt reorganized and enlarged the faculty of the College of Agriculture in 1912, several new divisions were created and the statement was publicly made that "the best men available" would be employed. After the reorganization was completed, the roster was partially as follows: Richard L. Adams, who had been Director of the private experiment station of the Spreckels Sugar Company, and later assistant to the General Manager of Miller and Lux Company, was professor and head of the Division of Farm Management; at present he is also President of the Farm Credit Board. Ernest Babcock, who had taught in the Los Angeles State Normal School, and held the positions of Instructor and Assistant Professor of Plant Pathology and Assistant Professor of Agricultural Education at U. C., was professor and head of the Division of Genetics. J. Eliot Coit, a member of our staff at Whittier and Riverside, was professor and head of the Division of Subtropical Horticulture. Thomas Francis Hunt was, for a time, acting head of Agricultural Extension. Norman D. Ingham, who had been in charge of the Santa Monica Forestry Station and made himself a specialist on eucalyptus (see California Agricultural Experiment Station Bulletin, written by him) and also employed at the Laffingwell Lemon Company, was connected with the management of the James Ranch in Fresno County, after a similar position at Atascadero, California. Henry J. Ramsey left us for the United States Department of Agriculture. Harold Powell declined a division headship at the University of California, and finally became head of the field department, an important part of the work of the citrus organization, the California Fruit Growers Exchange. Stanley S. Rogers was professor and head of the Division of Truck Crops, and afterward in charge of an important branch of the State Department of Agriculture. Clayton O. Smith, the only one in the whole list who remained a genuine plant pathologist, pursued a long and productive service in Whittier and at Riverside, where he still (1952) lives in retirement. Fred L. Yeaw was called back to his alma mater (Massachusetts Agricultural College) as head of the Department of Truck Crops, and afterwards engaged in commercial fruit and vegetable work in New Mexico and Mexico.

There are two other men whom one cannot refrain from mentioning among the pioneers of plant pathology in California. These are William T. Horne and Howard S. Fawcett, both of whom came here about 1908. Horne, a native of Nebraska and a graduate of the university of his native state, came to us from Cuba, where he was an experiment station plant pathologist. At the University of California his name is most associated with undergraduate teaching, which was his principal function and which he exercised with outstanding success. Of the fine boys and girls who studied plant pathology at the University of California previous to his removal to Riverside, all received their inspiration and first training from Professor Horne. His system of undergraduate research and theses started many embryo scientists on the road to professional status and their problems like Armillaria, strawberry yellows, brown rot of apricot, olive knot, and others which grew into important experiment projects. His interest in subtropical fruits other than citrus, like avocado, guava, feijoa, and so on, as well as in general horticulture, was well known. Personally he was a good friend and companion, probably the best loved and most generally liked member of the plant pathology staff at the University during its half century's existence.

One day in 1908 I was at our Whittier laboratory when a young man came in and introduced himself as Howard Fawcett. A native of Iowa and graduate in plant pathology of the state college there, he had just come from Florida where he had been working on citrus diseases for that state. Now he had been employed by our California State Commissioner of Horticulture for similar work here and was looking over southern California to select a location for his headquarters. One of the local colleges, where by chance the Commissioner was a professor, had been suggested. I know something which Fawcett did not, namely that the commissioner had a private grudge against the University and wanted to set up an institution in southern California under his office which would outshine our new pathological laboratory at Whittier, although he knew well that he had no business to do so.

I took a strong liking to Fawcett and believe that the feeling was mutual regarding our whole set up. After some general conversation and showing him around the new building and introducing other members of the staff, I asked our visitor, "Have you reached any conclusion about locating?" "Well," said Fawcett, the honest, straightforward Quaker, "I like it right here," and "right there" it was until he was transferred to the University and to Riverside. His great work on diseases of citrus is recorded in his book on that subject and in many other publications.

Early work in the 1900s relating to control of crop troubles in California was almost entirely limited to that performed by the University of California, but one of the most important activities of this sort was the work of C. Harold Powell of the United States Department of Agriculture on decay of oranges and lemons in transit. Powell and his associates showed that by careful handling and avoidance of bruising of oranges and lemons, enormous losses which had been occurring could be avoided.

In concluding this review of early plant pathology in California, it might be interesting to raise a question which is sometimes asked me, "Who was our greatest plant pathologist? You knew them all and should be able to tell." A question like this regarding a strictly economic subject raises many other questions as to what constitutes greatness and how should it be evaluated. I would say that greatness in this case is the ability to do things which make possible our understanding and control of plant diseases, which would include both technical discoveries and practical application. From this point of view, as well as general impressions, two men stand out in my mind above all the others, Pierce and Fawcett. Twenty years' time separated them (Pierce started here in 1889 and Fawcett in 1908) and it was twenty years of progress in knowledge and methods of plant pathology, so each must be judged against the background of his own time. Each had great energy, complete intellectual honesty and freedom from theoretical deductions, and a strictly factual approach to all conclusions. Each was modest, quiet, and sincere. Each determined the cause of an important bacterial disease (walnut blight, citrus blast), each showed the nature of a mysterious virus disease (Pierce's [California] vine disease, psorosis of citrus) and each obtained complete evidence on the cause and control of a fungus disease of major importance (peach leaf curl, lemon gummosis). Neither made any embarrassing mistakes or announced any erroneous conclusions. Pierce had the disadvantage mentioned above of working at an earlier period. Twenty years later I think without question he would have called the vine disease a virus when he wrote that the trouble was a true disease capable of spreading from vine to vine and not a transitory effect of environmental conditions, that no living organism had been identified as the cause, and "the observed phenomena would be in the main explained if there were a form of living organism *within the vine* capable of altering the normal physiological relations of the plant." (My italics)

It would be interesting to go on and describe the further development and great expansion of plant pathology in California which has taken place since the breaking up of my pioneer "ought five" group in 1912. I returned from a sabbatical in Europe at that time and found my whole staff gone except Miss Smith and Professor Horne. But the foundations were laid. Good new men developed from among our students, others were obtained from progressive institutions like Cornell University, University of Wisconsin, and Iowa State College. Work in plant pathology in California by other organizations like the United States Department of Agriculture and our State Department of Horticulture expanded; other Divisions in the College of Agriculture contributed, and thus our knowledge of the many new problems was kept fairly well in line.

And so, although I can see many and better things which could have been done in this past half century, I can still look back to that morning in 1903 and face the young man from Massachusetts as he came onto this campus for the first time, with the pride of honest effort and accomplishment for myself and every one of the associates who made this possible. May my successors be as fortunate as I.

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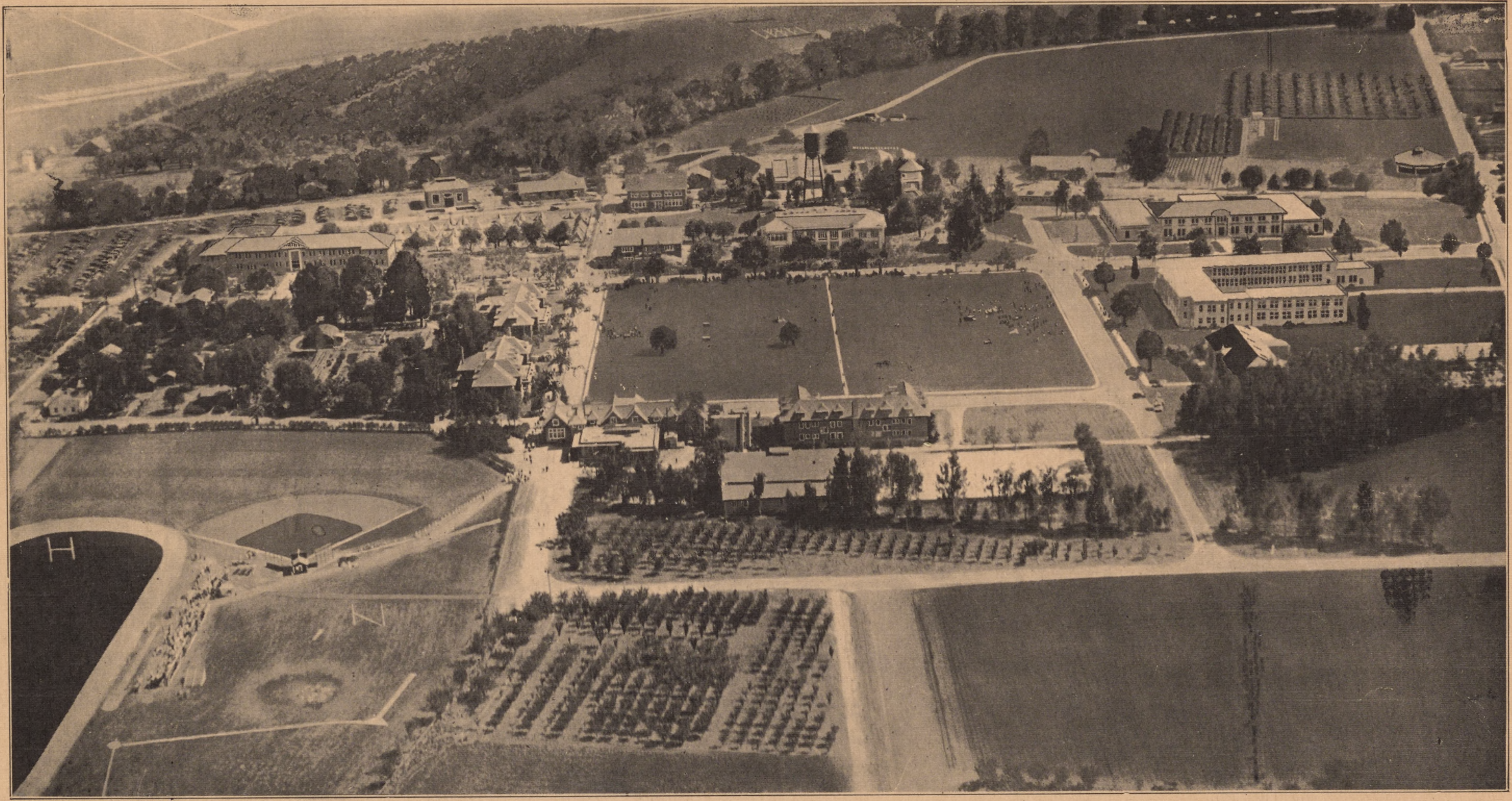

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